

Debate without Raised Voices

HOWEVER unsatisfactory may be the outcome of the discussion of the future of British science policy provoked by the publication of the government's green paper containing the Rothschild and Dainton reports (see *Nature*, 234, 169; 1971) there is very little doubt that the occasion will provide yet another demonstration of the gentility of British public life. So far, the people most affected have been muttering to themselves and to their friends about the iniquities of one or other (and sometimes both) of the two documents, but there has been no sign so far of the kind of intellectual debate which the issue deserves. Christmas, perhaps, may be a diversion, although there is every reason to hope that the scientific community will not put turkey and plum pudding before its more lasting interests. It is also to be hoped that when eventually people start responding to the government's plea that interested parties should have their say, they will do so in language free from the ritual cant with which the subject is usually blessed. Last week, Dr G. J. Leigh began a statement on the subject with the words "The Rothschild report and recommendations present a threat to the well-being of British science which must be resisted". And the annual meeting of the Medical Research Society on December 10 adopted a formal resolution that "this society disapproves entirely with the principles stated in the report by Lord Rothschild . . .".

The weakness in these formal statements of opposition to the Rothschild doctrine that research policy should be determined by the demands made by customers (government departments) on research councils and their establishments is that they imply that the conduct of scientific research is an autonomous process in which the balance of effort is not determined by the wish somehow to obtain practical benefits. In practice, however, the research councils most directly threatened by the Rothschild proposal—the Natural Environment Research Council, the Agricultural Research Council and the Medical Research Council—have all relied in recent years on the argument that their activities are of practical value. Moreover, nobody will dispute that immense benefits have been won from much of the work that has been carried out under the umbrella of the research councils—the strains of wheat which the Agricultural Research Council have developed have been, for example, a great practical and economic benefit. In other words, although there may be some who object as the Medical Research Society apparently does to the principle of the Rothschild report, most students of the question will agree that the overriding issue is not whether customers of one kind or another should make demands on the research councils but that of know-

ing how best these demands might be made. On this point, Dainton and Rothschild share common ground, for the Dainton report says that in the years ahead, "research councils will have to become increasingly well informed about national needs and objectives so that they may try to deploy scarce resources in the most appropriate directions and be seen to be doing so". In other words, the Dainton view is that the research councils should comprise enough skill and understanding of practical problems for them to be able to make coherent demands on laboratory scientists. What this implies is that the two reports differ only in the arrangements which they suggest for expressing the interests of the ultimate beneficiaries, the British taxpayers, in language that can be used to define a programme of research.

What kinds of questions should be asked of the research councils? Implicit in Sir Frederick Dainton's definition of strategic research, that kind of scientific activity considered likely to lead to beneficial results which is nevertheless unpredictable in character, is that there are limits inherent in the character of the scientific process to the precision with which goals can be defined in advance. So far as the Dainton report is concerned, it would be entirely proper for government money to be spent under the banner of strategic research on a comprehensive investigation of the physiology of domestic farm animals because of the near-certainty that in the decades ahead, a better understanding of animal physiology will be of great practical importance in agriculture.

As things have turned out, this is precisely the point of view which Lord Rothschild rejects. To him, such an investigation is far too unspecific. To him, the questions to ask are questions such as "Is it feasible to think of developing Christmas turkeys weighing substantially more than those at present on the market and, if so, what kind of a research programme would be necessary and how much would it cost?" This is exactly like the question whether it is feasible to think of developing a supersonic aircraft and, if so, what kind of a research programme would be needed and how much would it be likely to cost? The real issue between Rothschild and Dainton is not whether questions like these are valuable questions—and some, of course, say that they are indecent—but how extensive is the field of enquiry in which they can be useful guides to the development of research policy. This, as it happens, is an empirical question, not an issue of principle. Those who pretend otherwise are turning the debate on research policy (such as it is) into the intellectual equivalent of a mediaeval joust, with Rothschild and Dainton as tin-clad champions.



Another Notch in the Tightening Belt

THE publication of the British government's forecast of public expenditure between now and April 1976 which appeared a month ago (*Public Expenditure to 1975-76*, Cmd. 4829, HMSO, £0.68) is a solemn warning that, however the dispute between Dainton and Rothschild may be resolved, there are hard times ahead for scientific research and development. The object of publishing estimates of public expenditure well in advance is quite properly to provide a framework within which later political decisions about the direction of government policy may be determined, and it is only fair to say that this year the Treasury has done better than ever. It may be a long time before forecasting of public expenditure is done with as much detail as it might be, with the costs of alternative programmes shown separately, but the British government has at least gone a long way in spirit to meet the recommendations of the Select Committee on Government Expenditure a year ago. But even if there are plenty of opportunities for changing government policy on such things as money for the research councils or for various government establishments, the estimates of forward expenditure which have now been provided offer very little hope of a change in the scale of finance for research and development in Britain in the next two or three years. If anything, and if present tendencies continue, even less money will be spent in 1975 than in the present year.

In the current financial year, for example, the research councils are likely to cost £125 million—the Science Research Council will consume £56 million or roughly 45 per cent of all government expenditure on basic civil research. The forward estimates now published suggest that this sum will increase to £134 million in 1973-74 and £137 million in 1974-75. These figures entail reductions from previous estimates—for policy reasons, for example, the government now says that the research councils will have £3 million less to spend in 1974-75 than had been intended a year ago. Why should this be? The government says that it has tried to bring the growth of expenditure "more into line with that of public expenditure programmes as a whole". At the beginning of the next five years, the rate of growth of the combined budgets of the research councils will be about four per cent a year, but by the end of the period, the Treasury congratulates itself, the rate of growth will be identical with that of public expenditure as a whole. The result, of course, is quite predictable. Five years from now, the research councils will not be able to provide research grants and studentships in larger numbers than at present even though, by then, the university system will have grown by something like 30 per cent. Moreover, the government has plainly set its face against the concept of what is called sophistication—the tendency for ever more expensive and intricate equipment to become available for carrying out specific jobs. Only two years ago, the Department of Education and Science was estimating that, on this account alone, it would require that an extra four or five per cent a year should be made available simply so as to keep the then scientific labour force at work on basic research fully employed.

If the research councils are in for a hard time, so too are the research and development organizations supported by the Department of Trade and Industry. The white

paper now published reckons that the research establishments supported by the department will cost £61 million in the present financial year but merely £40 million four years from now, in 1975. The laboratories concerned include those of the Atomic Energy Authority as well as national institutions such as the National Physical Laboratory, the National Engineering Laboratory and the Warren Spring Laboratory. The white paper says that much of the saving of £20 million expected in the next five years will come about because the Atomic Energy Authority will be concentrating increasingly on fast reactors for the nuclear power programme, but it also says that the industrial research establishments and the Atomic Energy Authority are continually trying to recruit commercial work from outside the government. The implication is that the cost of nuclear energy research as such will decrease by about £10 million and that, if there is to be no physical contraction of the research establishments, the laboratories between them will have to raise something like £10 million a year in commercial contracts. The Atomic Energy Research Establishment at Harwell has been doing splendidly in the past few years, but it remains almost the only laboratory of its kind to have made substantial business as a commercial research undertaking. Even the most optimistic supporter of the doctrine of commercial business in government laboratories will be hard pressed to see how other establishments could grow as quickly in this direction as the white paper implies. It follows that the government must have at the back of its mind an actual decrease in the size of some of the establishments. The question it seems not to have asked itself is whether the time may not be coming when one of the most effective ways of spending public money is to devise ways of channelling funds towards industrial enterprises which are likely to be commercially successful in a comparatively short time. If Britain is ever to carry out the development of a reasonably successful typewriter or sewing machine, or one of the other products of profitable technology, the chances are that the government itself will have to take a lead.

To protest that changes like these, spread as they are over the next five years, represent an attack on scientific research and development would of course be foolish. Quite possibly, the government is calculating that decreases in public expenditure will be more than compensated for by increases in the amount of money spent by industrial companies but the statement last month is entirely silent on that subject. Moreover, it is clear that the government intends to reduce as far as possible such things as its support for industrial projects of other kinds—both the National Research Development Corporation and the computer companies which have benefited from government support in the past few years are in for leaner times. The difficulty in all this is of course that nobody has ever thought out the scale on which the British government should support scientific research and development both through the research councils and in industry. In spite of all the fuss now under way about the Rothschild proposals, these are strictly concerned with organization. Nothing is said about the scale on which expenditure would be desirable and, for that matter, nothing is said about the

framework within which public support for industrial research as distinct from the kind of research which the research councils carry out should be undertaken. But these are small parts of the government's interest in research and development.

Last year, defence research accounted for 40 per cent of all government expenditure on research and development and although the proportion has been falling steadily for several years, it is hard to think it wise for expenditure on such a scale to continue much longer without a thorough investigation of the principles on which the money should be allocated to different causes. The result of all this, of course, is the paradox that while the scientific community in Britain has its interest chiefly engaged in an important but essentially organizational question about the best way of allocating funds to one part of publicly supported civil research, the government is perpetrating policy decisions about the scale of its involvement in research and development as a whole in the years ahead which have serious consequences for the scientific community and which may even be unwise as well.

Fishing in European Waters

FISHERMEN are understandably among the most vociferous and conservative members of any community, which is no doubt why it has taken such a time for the government now seeking membership of the European Communities to reach an understanding on fishing policy. On December 12, the representatives of Britain, Ireland and Denmark seem to have reached an accommodation with Brussels on a fishing policy for the next decade, but the government of Norway will come back next year to resume negotiations which are plainly turning out to be so complicated that Norway may yet finish up outside the European Communities. For the remaining countries, the new agreement on fisheries is in fact more than generous.

Unhappily, there is nothing in this agreement to ensure that the decade ahead will be one in which fish in north-west Europe are free from the pressures of over-fishing which are at present a far greater danger to their survival than any other influence. In the years since the Second World War, herring have been seriously damaged by over-fishing, which is why it is encouraging that the North-East Atlantic Fisheries Commission apparently agreed at its meeting in Moscow last week to impose not merely a closed season on herring fishermen but also restrictions on the permissible catch as well. Only time will tell if these new proposals will be less toothless than the regulations with which the International Whaling Commission has failed to protect blue whales in the South Atlantic, but there is always room for hope.

The long haggle in Brussels over the precise regions of the European coast to be fished by vessels of different nationalities has more acutely drawn attention to the folly of attempts to conserve the stocks of fish in the waters off north-west Europe by the arbitrary methods now sought. A part of the trouble, of course, is that even the agreed twelve mile limit does not necessarily contain all the breeding grounds where most damage might be done by over-fishing. If it is intended that there should be a European fisheries policy, it should at least cover the whole of the North Sea and those parts of the Western

Approaches which are accessible to rational study. The second difficulty which has emerged in the past few months is that very little is known of the biology underlying the productivity of the European fisheries. Is it possible, for example, that the remarkable increase in the numbers of young haddock and whiting in the North Sea within the past decade is to be linked with the way in which the herring have declined in the same period? The species do not compete for a common food supply but there may be other influences at work. And why is it that the Plymouth herring has chosen this time to return in strength? Is there some secular change of climate? Is it possible that pollution, on the moderate scale on which organic nutrients find their way into the North Sea and similar basins, is actually a help and not a hindrance? These are all questions which need to be much more adequately understood before a permanent fisheries policy is negotiated, and for this reason it is entirely welcome that Mr Geoffrey Rippon should have told the House of Commons last week that he had urged on the European Communities "the need for common action in all our interests to ensure conservation of stocks". Unhappily, so far as can be told from government departments, Mr Rippon has no specific proposals for setting up machinery to regulate fisheries on a European basis. It is to be hoped that he and his colleagues will not wait for the whole decade before clearing their minds.

100 Years Ago



MANCHESTER

Literary and Philosophical Society, November 28.—Dr. J. P. Joule, F.R.S., vice-president, in the chair. "Encke's Comet and the Supposed Resisting Medium," by Professor W. Stanley Jevons. The observed regular diminution of period of Encke's comet is still, I believe, an unexplained phenomenon for which it is necessary to invent a special hypothesis, a *Deus ex machina*, in the shape of an imaginary resisting medium. I cannot be sure that the suggestion I am about to make has not already been made, but I have never happened to meet with it; and therefore I venture to point out how it seems likely that the retardation of the comet may be reconciled with known physical laws. It is asserted by Mr. R. A. Proctor, Prof. Osborne Reynolds, and possibly others, that comets owe many of their peculiar phenomena to electric action. I need not enter upon any conjectures as to the exact nature of the electric disturbance, and I do not adopt any one theory of cometary constitution more than another. I merely point out that if the approach of a comet to the sun causes the development of electricity arising from the comet's motion, a certain resistance is at once accounted for. Wherever there is an electric current, some heat will be produced and sooner or later radiated into space, so that the comet in each revolution will lose a small portion of its total energy. In the experiments of Arago, Joule, and Foucault, the conversion of mechanical energy into heat by the motion of a metallic body in the neighbourhood of a magnet was made perfectly manifest. If then there is any magnetic relation whatever between the sun and the comet, the latter will certainly experience resistance. The question is thus resolved into one concerning the probability that a comet would experience electric disturbance in approaching the sun. On this point we have the evidence now existing that there is a close magnetic relation between the sun and planets. If, as is generally believed, the sun-spot periods depend on the motion of the planets, a small fraction of the planetary energy must be expended.

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OLD WORLD

MUSEUMS

Money for Monuments

A GOVERNMENT grant of £200,000 a year to assist in the purchase of museum objects of industrial and technological interest is recommended by the Standing Commission on Museums and Galleries in a report published last week. It is recommended that the money should be spent in circumstances where at least 50 per cent of the cost of preserving the particular object can be obtained from other sources (*The Preservation of Technological Material*, HMSO, £0.365).

The money would be administered by an advisory committee to be set up by the Department of Education and Science and supported by a secretariat in the Science Museum, with an assistant keeper at its head, which would also offer assistance to other bodies on all aspects of the preservation of all types of technological material.

The working party says that the Department of the Environment, which has responsibility for *in situ* historical monuments in England, should spend more on their preservation while the Secretaries of State for Scotland and Wales should also endeavour to do the same for monuments in their respective countries.

The working party to investigate the problem of the preservation of technological material was set up in 1970 by the previous government following a decade of mounting concern about the relics of technological developments in Britain. Several monuments have been lost in recent years—in several instances because

there was not enough money available to purchase them in the limited time before they were to be destroyed. As typical examples the Sudbrook Engines that were used until 1967 by the Great Western Railway and then British Rail to pump the Severn Tunnel were at that time due to be scrapped. British Rail offered to preserve them *in situ* for a sum of £70,000 but as the money was not available they were destroyed.

At the moment there are some monuments that have been saved but which need a lot of money to restore them to their original condition. The SS Great Britain is now in a Bristol dry dock where she was built in 1843 and there are frenzied efforts in progress to raise money to restore the ship to her former glory. All the money so far has been obtained from a private benefactor and in spite of the interest shown by the iron and steel industry in preserving the ship there is still a need for more money.

There is an even more pressing need for financial contributions to save other relics of the Industrial Revolution. The Iron Bridge, in the village of that name in Shropshire, the first cast iron bridge in the world, is in need of funds to rectify the subsidence of two of its abutments. Unless money is available soon, it will not prove possible to preserve the section of the Menai Bridge that was badly damaged by fire in 1970. Temple Meads railway terminal, Bristol, designed by Brunel is the oldest remaining main line rail terminus in the world. It has three times in recent years been given a lease of life—the last time earlier this year when British Rail were refused permission to build an office block on the site of the terminus.

The secretariat to administer the

£200,000 would be concerned with industrial monuments, relics of industrial social life, ships and relics of the shipbuilding and merchant shipping industries, art connected with industry and archives, including photographs and sound and film archives. The secretariat would consist of representatives of museums, Department of the Environment, as well as individuals who have knowledge of the history of technology. The working party recommends that the secretariat should have help when necessary from representatives of other bodies, such as the Public Record Office and the Royal Commission on Historical Manuscripts.

SPACE

Britain Bows Out

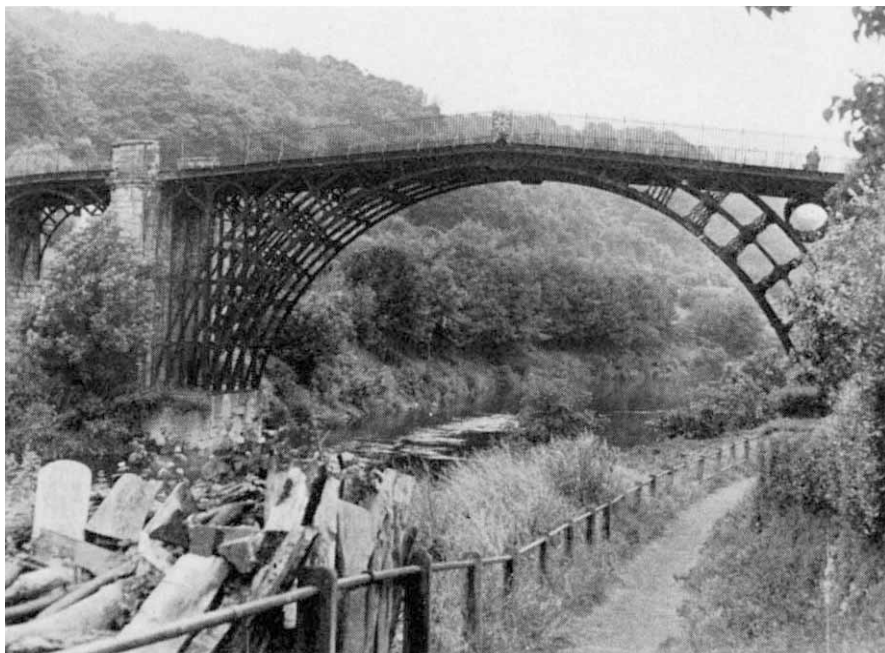
BRITAIN's request to withdraw from the European Launcher Development Organization (ELDO), made in Paris last week, means that Britain no longer has even a nominal interest in launcher development. The decision to withdraw is the logical result of the announcement in 1968 that Britain would not support an extension of the ELDO programme and would limit her financial commitment to £11 million from January 1, 1969.

The decision ties in with the scrapping of Black Arrow earlier this year (see *Nature*, 234, 4; 1971) and underlines Britain's belief that the launcher needs of European space programmes can best be met by buying rockets from the United States. Britain is confident that American Scout rockets will be available whenever needed for her own satellite programme, and only last month the US State Department agreed in principle to provide launch facilities for the member countries of ELDO.

Other European nations, notably France and West Germany, are less happy to rely on the Americans for launchers and feel the development of a European rocket to be a necessity. The consistent failure of the Europa I launches, however, followed by the spectacular failure of Europa II last month does suggest that the development of a hybrid multinational rocket may not be a realistic possibility.

Britain's withdrawal, which due to ELDO's constitution will not become effective until January 1973, does not affect Hawker Siddeley Dynamic's contract with ELDO to supply Blue Streak, which forms the first stage of both Europa I and II.

Now that the United Kingdom is free of the shackles of a European rocket that does not work and a national one that was not sufficiently powerful, the £19 million spent on civil space in 1970–71 can be used for such things as satellite developments, both nationally and within Esro.



The Iron Bridge at Iron Bridge, Shropshire

FOG CLEARANCE

No Clear Skies Yet

Fog is expensive. It costs time, money and lives. In just one nose to tail pile up in Britain last month, nine people died and fifty were injured. One day's fog at Christmas when planes are full could cost BEA alone £40,000, although money saved from cancelled flights would bring this figure down. Yet comparatively little research is done in Britain into fog dispersal.

Fog dispersal, whether of warm or cold fogs, is beset with a number of problems. Any dispersal process must operate continuously as mixing and drift within the fog rapidly fills in cleared spaces. Dispersal methods must not in themselves create equally serious hazards, and any method for frequent dispersal must be economically viable.

Supercool fogs are the easiest to disperse. Once the fog is below -4°C silver iodide crystals, liquid propane or CO_2 can be dropped into it from aircraft, cooling the droplets so that they fall as snow. Alternatively, material can be forced into the fog by generators at ground level. Dispersal by these methods is used in Norway and North America and they are established techniques, the economics of which are viable if fogs are frequent.

Warm fogs, however, pose greater problems. During the war a system known as FIDO was used in Britain to improve visibility for returning bombers. This involved the burning of tons of oil along the runways. After the war it was abandoned in Britain on the grounds of cost and no commercial fog dispersal has since been attempted. FIDO was still used at Los Angeles airport until 1953, however, when cost and pollution put a stop to its use. It has been reconsidered in various forms from time to time—the latest suggestion being that North Sea gas should be used at Heathrow—but it is an extremely expensive system, causing severe turbulence around the runways, and flying through the ring of flame has been compared to going through hell. At Orly airport, near Paris, an Atar 101F jet engine has been installed on one runway, the heat of which evaporates fog. The system is considered semi-operational and to date no turbulence problems have been encountered, although only light aircraft have used the runways.

Successful dispersal has been achieved in America by flying a helicopter over the fog using the downdraft to blow the dry air above the fog down into it, thus drying the fog out. This method is also expensive, however, and only the Americans have helicopters capable of producing a large enough downdraft.

The approach that the Americans most favour, however, is seeding.

Hygroscopic material is dropped into the fog to effectively dry it out. Common salt can be used, but the problems of corrosion that result from dropping tons of salt on runways may prove prohibitive. A number of American companies have taken out patents on chemical and detergent mixtures which, they claim, increase the cohesion of the droplets but the scientific basis for this has not been proved and in Britain it is doubted if these offer a real solution. The cost and effectiveness of both these approaches are questionable. Dr B. A. Silverman, of the USAF, estimated recently that to clear a fog over one runway, for one hour, to allow aircraft sufficient visibility to land would require the use of several large aircraft to seed-drop up to 100 tons of material at a cost of between \$30,000 and \$250,000 an hour. Dr Silverman predicted that in three to five years these systems will be in operation, but British researchers consider that a rather optimistic estimate.

Research in Britain concentrates on the fundamental microphysics of fog as it is felt that fog dispersal systems cannot be effectively assessed until fog is fully understood. In the United States research is concentrated on actual dispersal methods and considerable success is claimed for some of them. Earlier this month Northwest Environmental Technology was granted \$159,000 by the Federal Aviation Administration to measure and evaluate seeding techniques, and Dow Chemical Company is part way through a \$99,000 research programme.

For the immediate future, however, airlines prefer to put their money into blind landing systems as in reality economic warm fog dispersal is not just around the corner. The clearing of fog on motorways is completely out of the question for years to come, partly on the grounds of cost, and because of the impossibility of keeping the cleared patch where it is wanted.

EDUCATION

Teachers Taught

THE Royal Society's scheme to involve school teachers in academic research is now in its fourteenth successful year. The latest report of the society's Scientific Research in Schools Committee shows that 102 teachers are investigating diverse topics under the supervision of Fellows of the Royal Society and other eminent scientists. And as in previous years, several of the teachers have published their results in such auspicious organs as the *Journal of Animal Ecology* and *Spectrochimica Acta*.

When the committee was set up in 1957, thirty-three research projects were

started in schools, with the pupils being encouraged to participate whenever possible, and by 1966 there were 107 projects in progress. One of the greatest benefits to the teachers is that this scheme can provide them with an enthusiastic and knowledgeable adviser who will often lend facilities and equipment as well as expertise. In this way some school teachers are able to continue work which they started while at university, and others can gain their first taste of research.

The committee is able to give financial help in the form of grants for equipment, sometimes as large as £600. As well as the Royal Society's own funds, the sources of this money include the United Kingdom Atomic Energy Authority, which has given up to £1,000 annually since 1963, and several large companies.

Last year grants totalled £2,250, but the decline from the £4,500 spent in 1969 is not a consequence of economic constraints; the projects on hand at the moment are just more modest in their requirements. They do not, however, seem to be any less ambitious. Topics under investigation in British schools include the application of gas chromatography to the study of reaction kinetics; the reproduction of the American cockroach; thermal degradation of phosphonitrilic isothiocyanate; erosion processes in the New Forest; and limiting factors in mice populations.

MACROBERT AWARD

Gas from Naphtha

THE MacRobert Award for 1971 has gone to a team of five researchers from the Gas Council for their work on gas manufacturing processes. The £25,000 award, which was instituted in 1969, goes to Dr F. J. Dent, formerly Director of the Gas Council's Midlands Research Station, and the team who worked with him—Dr Dennis Hebden, Mr George Percival, Dr Brian Thompson and Mr Ronald Edge.

The award is made for an outstanding innovative contribution in the fields of engineering, technology or the applied physical sciences which has enhanced or will enhance the prestige and prosperity of the United Kingdom. Previous winners are Freeman, Fox and Partners for their design of the Severn Bridge, Rolls-Royce (Bristol) for the Pegasus engine and BP for its exploration of the Alaskan North Slope Oilfield.

Dr Dent's team developed five processes of gas manufacture, one of which, the conversion of naphtha into towns gas or natural gas substitute, has already earned the Gas Council £0.5 million with a further £2 million to come and more orders expected, principally from America and Japan.

LEVERHULME TRUST

More than a Million

THE Leverhulme Trust spent £1.97 million during 1968–70 on grants to individuals and to charitable institutions. About 65 per cent of the funds available went to educational bodies and £150,000 was distributed in the form of personal studentships and fellowships. And the annual income of the trust, which was set up on the death of the first Lord Leverhulme in 1925, exceeded £1 million for the first time in 1971. The trustees actively encourage projects involving interchange between academics in different countries and about 25 per cent of the total expenditure for the three years was on schemes of this type.

Spending by the trust on what it calls "basic sciences and mathematics" during the three years amounted to £237,000 (Table 1), about 9 per cent more than in 1965–67. Many of the grants went to universities in the form of research fellowships but money was also allocated for the establishment of units within universities. For example, a grant of £31,000 over five years was made to the Institute of Animal Genetics at the University of Edinburgh to help the establishment of an international school of molecular biology; six studentships are to be provided each year for students from Edinburgh to work at the University of Naples and for both lecturers and students from Naples to spend time at Edinburgh. A new unit in social statistics was also set up at the University of Southampton at a cost of £36,000 over four years.

In the applied sciences the chief feature of 1968–70 was the decision of the trustees to make £10,000 a year available for five years to pay for up to five visiting fellowships in industrial biology. The response of individual universities to the proposal was not very encouraging at first but the Science Research Council agreed to administer the scheme because it seemed to fit in with the needs expressed by its own industrial biology working party. Holders of the fellowships—there were two awarded in 1970—spend at least sixteen weeks a year in a university department with the aim of promoting a keener sense of the requirements of industry.

The trust's expenditure on "combined

subjects" has included the setting up of courses and fellowships in music and physics, archaeology and metallurgy, and archaeology and physics. Prominent among the grants for medical and dental projects has been the allocation of £60,000 to the Institute of Ophthalmology, University of London, for a unit to study retinitis pigmentosa—a chronic progressive inflammation of the retina.

Research projects in the environmental sciences that have been funded by the trust include a research unit in urban design at the University of Edinburgh and a unit for a study of the location of industry at the University of East Anglia.

SCIENCE MUSEUM

Games People Play

THE Science Museum last week opened an exhibition on scientific toys designed to appeal to both adults and children. The toys range from an eighteenth century orrery to a modern Newton's cradle for frustrated executives which sits inside its glass cage clicking patiently back and forth.

Many of the toys work, and demonstrate various scientific principles from the effects of atmospheric pressure to the use of photoelectric cells. An air fountain with its jet adjustable for height and direction spins coloured pingpong balls in one corner, while at its back a water fountain is operated by thermal pressure. Across the far side a magnetized triangle dances above a triangular layout of magnets as their opposing poles repel it and a mechanical yoyo rolls stolidly up and down. Among the older toys is one of the largest toy steam engines ever made—in Germany in 1906—the dynamo which it powers being used to light the bulbs on a toy Christmas tree. The commentaries round the exhibits include background information and claim that toy manufacturers are rarely far behind scientific advance—a toy do-it-yourself X-ray kit for practice on the family was marketed in Italy early this century before the dangers of X-rays were known.

The exhibition, which will run until March, also includes one or two ideas for re-creating the exhibits in the home. The air fountain, it is suggested, could be constructed with pingpong balls and a vacuum cleaner which can be persuaded to blow rather than suck.

HIGHWAY PLANNING

Marsh's Crystal Ball

THE forum on Public Participation in Highway Planning at the Institution of Civil Engineers last week did little more than to illustrate the gap between the planners and those planned for, and the stuttering inability of public meetings to

follow a logical line of thought. Several hundred people filled the Great Hall of the institution to hear questions from the floor answered by four experts in highway planning under the chairmanship of Mr Richard Marsh, Chairman of the Railways Board and ex-Minister of Transport.

The experts were Mr Michael Heseltine, Parliamentary Under-Secretary responsible for motorway and trunk road routes, Mr Frank Layfield, QC, counsel specializing in highway matters, Mr Derrick Beecham, founder of the Homes before Roads campaign, and Mr A. Goldstein, an engineer and member of the Roskill Commission.

Mr Beecham was worried that petrol bombs might be thrown at builders unless politicians listen but the evening's discussion did little more than ignite a few rather damp and well worn squibs.

Any constructive line of argument was defeated by speakers from the floor airing particular grievances or changing the tack of the argument. Complaints were made about inadequate compensation and lack of compensation for people living near motorways. Mr Goldstein warned of the dangers of extensive property blight if detailed plans of all possible routes are published for discussion as some wanted. Mr Beecham and speakers from the floor argued that the public should be consulted before detailed plans are drawn up, and even before the decision is taken over whether the money should be spent on roads at all, rather than on rail.

But do the public want to be involved? Mr Heseltine said that most people are not interested unless a motorway is likely to plough right through their back garden. Mr Heseltine also argued that from the letters he received, the vast majority of people want new roads.

But while there were plenty of suggestions as to when in the planning process there should be consultation, nobody had much to say as to how this should happen, other than Mr Beecham's suggestion of "some sort of a referendum". How a referendum should be held on the plan of future roads for Greater London was not explained.

Mr Marsh, in summing up, said the meeting showed "what we already knew"—that people are worried about what is being done to the environment. There was, he said, no single and simple answer to the complex problem of road planning. Now freed of the shackles of ministerial responsibility, Mr Marsh assured the meeting that ministers are responsive to public pressure and further that if it builds up, "the government of the day will not change their mind because of it, they will beat (the public) to it and change in advance of it". No doubt, across Parliament Square in Marsham Street, Mr Peter Walker, Secretary of State for the Environment, was busy polishing his crystal ball.

Table 1 Expenditure on Science 1968–70

	£ thousand
Basic sciences and mathematics	237.1
Medicine, dentistry and health	272.3
Applied sciences, engineering and architecture	91.1
Environmental sciences	99.6
Combined subjects	8.7
	708.8
(35 per cent of total expenditure)	

NEW WORLD

Geared for Action

by our Washington Correspondent

NEXT week, between five and ten thousand scientists will converge on Philadelphia for the annual post-Christmas extravaganza organized by the American Association for the Advancement of Science. Part scientific meeting and part public relations exercise for the wonders of science, the annual meeting of the AAAS has for most of the past 138 years served to convey to an admiring public the fruits of scientific research and the flavour of the scientific enterprise. But, like the annual meeting of the British Association, that established order is slowly changing.

For one thing, scientists no longer save up their cherished results to reveal to the world at the annual meeting of the AAAS—that is now the province of scientific journals, or to a lesser extent of more prestigious meetings such as the annual meeting of the American Chemical Society. And, for another, the whole concept of scientists condescendingly handing out fruit from the tree of scientific knowledge to a grateful public (which is in any case indirectly paying for the research and is profoundly affected by it) is anathema to many scientists, and not just those on the far left. Consequently, the AAAS meeting is attempting more and more to look at the complex inter-relationships between science and society, and the meetings are increasingly becoming the target of radical scientists who, campaigning under the slogan "science for the people", flamboyantly attack members of the scientific establishment for their elitism and "crimes against the people".

The past two meetings of the AAAS have caught headlines across the United States largely because of the tactics of the radical dissenters, and this year's meeting promises to be no exception. According to one member of SESPA (Scientists and Engineers for Social and Political Action, the organization chiefly responsible for coordinating the dissenters at the past meetings), for example, "the turnout (of radicals) will exceed any one in the past", and according to another, radical scientists will be more in evidence in Philadelphia than they were in Chicago last year.

By their tactics at previous meetings, and widely publicized incidents such as an attack on one radical dissenter by the enraged wife of a biology professor armed with a knitting needle, the objectives of the radical scientists have been obscured and their antics have served chiefly to alienate even many of those scientists who sympathize with some of their objectives. Will this year's meeting see a change in tactics? Herb Fox, a

Boston scientist who was active in Chicago last year, and who has been heavily involved in planning SESPA tactics for this year's meeting, said tactics will be chosen to suit the occasion. "We plan to participate in the discussions in a helpful way," he said, but warned "if the AAAS chooses to stop meetings, we shall kick up a stink about it".

For their part, those involved in planning the AAAS meeting point out that they want to encourage participation, and that the menu for the meeting consists of a range of topics from hard science to those where the social content is high. A particular feature of the meeting will be a series of discussions of the role of science in meeting urban problems, particularly in Philadelphia, and there will also be sessions on conflict in Northern Ireland and on "the new radicalism in science". The AAAS even offered SESPA members a hand in planning part of the programme, a move which was rejected by the organization because it might be interpreted as a sign of support for the AAAS, and because it would compromise SESPA tactics at the meeting. Nevertheless, some members of SESPA, Fox included, will take part officially in the meetings, as individuals.

Has the AAAS decided to adopt its own tactics to forestall the protests of the radicals? According to William Bevan, executive officer of the AAAS, the society always prepares a set of operating rules for the chairmen: "We don't play things by ear". But he pointed out that everybody will be free at the meetings to put his own point of view. S. Fred Singer, chairman of the AAAS meetings committee, said that chairmen have been told not to allow bull horns, megaphones and so on into the lecture halls. They have also been given explicit instructions not to close meetings until people have had their say, but that if the meeting becomes unmanageable, they have the power to adjourn—such a decision could be put to a vote, he said.

Singer said that he expects less disruption of the meetings this year, because there will not be so much television coverage—"it is my feeling that many of these people were grandstanding for the television cameras", he said. Fox, however, says "we have done something for the AAAS which they have always wanted to do for themselves, get their name widely known". At least the 1973 meeting of the AAAS will be more removed from the attacks of radical American scientists—the annual meeting is to be held in Mexico City that year.

ARTERIOSCLEROSIS

Another Crusade

by our Washington Correspondent

A MUCH larger national programme aimed at improving prevention and treatment of arteriosclerosis and at stimulating research into its causes has been recommended by a task force set up under the auspices of the National Heart and Lung Institute. Although officials of the institute flatly deny that the report is "a counter-balance or a gimmick to compete with the cancer attack program", the report is aimed at launching a crusade against cardiovascular disease in much the same way as the report of the panel of consultants a year ago launched a crusade against cancer. Moreover, the forthcoming debate about the merits of instituting the report's recommendations will be an interesting lesson in the way Congress and the Administration will set about dealing with large but scientifically and emotionally strong claims from different sectors of biomedical research.

Prepared by a fifteen member task force under the chairmanship of Dr Elliot Newman of Vanderbilt University, and later of Dr Oglesby Paul of Northwestern University, the report calls for a programme which would cost \$120 million in the first year and \$175 million in the second year above the present funding of \$104 million for research and treatment of arteriosclerosis. The basis for this request is that arteriosclerosis is the number one killer in the United States, and that there already exists a strong base of knowledge which would allow programmes aimed at preventing the onset of the disease to have a significant effect on mortality in the United States.

Throughout the Congressional debate on cancer, it was repeatedly emphasized that the disease afflicts about one person in four, but that argument is considerably strengthened when applied to arteriosclerosis, for the task force points out that arteriosclerosis is directly or indirectly responsible for nearly half the deaths in the United States each year. The disease, which is essentially hardening of the arteries caused by fatty deposits on their inner walls, is most prevalent among individuals having one or more of three specific factors—high levels of blood lipids, high blood pressure and cigarette smoking. The task force therefore sees the chief aim of any Federal programme as an intensive effort aimed at public education linked with an expanded research effort aimed at discovering more precisely the links between various factors and onset of the disease.

That arteriosclerosis is caused in large measure by extraneous factors such as

diet, habits and perhaps environmental factors can be inferred from the huge discrepancies in the prevalence of the disease among different countries. The United States ranks second in the international ratings and it occurs ten times more frequently there than in Japan. This discrepancy led Dr Charles K. Friedberg, a member of the task force, to suggest last week that if Americans were prepared to alter their diets and stop smoking cigarettes, the incidence of the disease might be cut by 90 per cent.

Among the chief recommendations of the task force are the following:

- Several centres devoted to the study and treatment of all factors of arteriosclerosis should be established in existing hospitals and universities. At present, the task force reports, "the effort is fragmented into small programs at many universities and hospitals. These programs, excellent as many of them are, do not encompass a multidisciplinary attack on arteriosclerosis". Called National Centers for the Prevention of Arteriosclerosis, the centres would be focal points for the study, detection, prevention, arrest and reversal of the disease, and they might be modelled on other mission-oriented government research centres such as the Lawrence Livermore Laboratory or the Oak Ridge National Laboratory.

- Smaller cardiovascular disease prevention clinics should be established within the framework of existing health care. Charged with the task of screening persons to detect those most at risk, the clinics would also serve as a means of collecting together information on the prevalence of the disease and its dependence on different factors.

- The task force also suggests that an Office of Health Education should be established in the National Heart and Lung Institute to serve as a clearing house for information on arteriosclerosis. The office would be partly responsible for public education about the risks associated with, for example, cigarette smoking, and about the advisability of having regular checks of blood pressure.

- As far as research is concerned, the task force suggests that "efforts aimed at primary prevention of arteriosclerosis are not based on adequate information", and that "a vigorous effort of basic and applied research will provide the knowledge required for efficient prevention programs". The research would be carried out primarily in the suggested National Centers for the Prevention of Arteriosclerosis, although the report suggests a large scale programme of applied research to test the so-called risk factor hypothesis. The chief problem with such trials, however, is that they would involve large numbers of patients to be studied over long

periods of time, but with the increase in the number of patients under treatment in the national centres, the task force believes that several important questions concerning the causal relationship between factors such as high blood lipid content and high blood pressure can be resolved.

- The task force also recommends that the National Heart and Lung Institute should engage in an expanded programme of basic research to elucidate, for example, factors governing the levels of lipids in the blood, and the mechanisms whereby genetics, hypertension and cigarette smoking affect the formation of fatty deposits on arterial walls.

Dr Paul has described the recommendations of the task force as "a broad, aggressive, realistic program deserving the support of the President, the Congress and of the people". Since the NIH budget is now being negotiated in the Office of Management and Budget, such a report could conceivably have an effect on the institute's funds for next year, and if the report does launch a campaign designed to cut down the incidence of the disease, it is likely to find many friends in Congress. One major difference between this report and last year's report of the panel of consultants on cancer, however, is that the argument is unlikely to be side-tracked into one of organization, because the task force thankfully makes no recommendations for taking arteriosclerosis research outside the National Institutes of Health.

OCEAN MAMMALS

Not Enough Protection

by our Washington Correspondent

UNDER the blind eye of the International Whaling Commission, eight species of whales have been ruthlessly hunted to the verge of extinction. Commercial greed has been the chief factor which has forced the commission's weak and toothless machinery consistently to allow whaling fleets to kill off more whales each year than their rapidly declining numbers can sustain. And, as a monument to bureaucratic controls that allow too much leeway to the commercial interests they are designed to regulate, the International Whaling Commission provides a good example of what not to do. That is one reason why the House of Representatives has chosen to throw out a bill designed to protect all ocean mammals, but whose secondary aim is to "obtain an optimum sustained yield" of the animals for commercial exploitation.

In many respects, backers of the bill were hoist with their own petard, for the bill was brought to the floor of the House under suspension of the rules—

a device designed to expedite the passage of uncontroversial legislation which allows no amendments to be brought, and which requires that the bill receives a two-thirds majority to pass. Those who wanted to give the bill more teeth were therefore forced to vote against it, and it received an absolute but not a two-thirds majority.

The bill was voted down essentially because it was felt that the shameful history of marine mammal conservation, which has allowed species after species to become almost if not entirely extinct, requires the taking of more drastic action than simply improving the controls on the giving of licences for hunting the animals. In short, what most opponents of the bill want is a complete moratorium on the killing and harassing of all ocean mammals by United States citizens, and a ban on imports into the country of products derived from these animals (the products are mostly luxury goods).

But the bill brought to the floor of the House by the Committee on Merchant Marine and Fisheries, although it would have led to a moratorium on the taking of some particularly endangered species of marine mammals, also included provisions allowing the Secretary of Commerce or the Secretary of the Interior to grant permits for the taking of other marine mammals. Before such a permit could have been granted, the bill specified that the likely effect on the species would have to be taken into account, and the whole process would be open to public hearing. The granting of such permits would, however, be subject to commercial pressures, and with the glaring example of the International Whaling Commission to hold up, opponents of the measure had enough big guns to shoot it down.

One of the chief reasons that the bill would have allowed the taking of some species of marine mammals is to permit effective management of mammal populations which might become too numerous for their habitats—the situation in the Farne Islands, where the British policy of stopping the killing of seals has led to overcrowding, disease and starvation among the animals was cited as an example. But in the majority of the cases—for example, the wanton destruction of polar bears in Alaska by hunters in the name of "sport", and the commercial exploitation of whales to the point of their extinction—the situation is such that strong controls are essential.

Although the bill has been technically killed by the vote, it may be resurrected and brought back to the House under the normal procedure, thus allowing amendments that would put the necessary teeth into the legislation. That is what most of those who voted it down are hoping.

Safety from Rothschild in Numbers

This is the second in a series of comments on the Dainton and Rothschild reports. The author is Professor C. H. Waddington, Institute of Animal Genetics, University of Edinburgh.

It is either frivolous or a symptom of a fixation on academia to suggest that reorganization of the research councils is a panacea for curing the difficulties of British high-technology industry. According to the Rand Corporation both pure and applied research amounts to no more than 5 per cent of the total costs involved in American high-technological products, the remainder being devoted to production development and product application in relation to the market. In the Dainton Report (Appendix D, Table 2), the research council contribution for the last year quoted (1967–68) was only £57.1 million out of £962.1 million, that is of the same percentage as the USA (though Dainton's total is increased by including defence research and lowered by excluding some of the marketing costs included in the Rand figures). Moreover, although 95 per cent of development has been run on the customer/contractor principle which Rothschild favours, it would seem, in view of recent British experience with advanced aircraft, rocket launchers, nuclear power plants, computer hardware and the like, more in need of radical reorganization than the 5 per cent spent on research. The research councils have every reason to refuse to accept the role of scapegoat. They have done, at a modest fraction of the GNP, a pretty good job—*vide* international comparisons of Nobel Prizes *per capita* as a crude but not negligible index.

Rothschild's recipe is straightforward. The principle is that the people responsible for running the affairs of society, that is the civil servants in executive ministries, know best what new knowledge or expertise is required. This means not only that "the person who has to pay . . . is in the best position to know how much he can afford" (Rothschild, para. 4), but also "the customer says what he wants" (Rothschild, para. 6). This short-circuits one of the traditional levels between customer and contractor. A sick man is usually wise to consult a doctor before going to the chemist for some pills; it is sensible to have an architect between a client and a builder. It is true that Rothschild wants each customer ministry to have a chief scientist (presumably with a staff, though little is said about this). But this has been urged since the end of World War II, with little or no effect. There are three snags.

First, unless the ministry's chief scientist has a fairly large, wide ranging

staff, he will simply perpetuate out-of-date scientific orthodoxy. For instance, when I started running a research laboratory for the ARC on genetics in the livestock industry, the ministry, with good scientific advice (from veterinary surgeons), knew just what it wanted—more control over the results of matings between pedigree bulls and cows. Really it wanted something totally different, belonging to a different paradigm—to forget about the Breed Societies, and find out how to operate the Artificial Insemination Centres in terms of hundreds of thousands of matings. But if you were to set up a ministerial science office capable of seeing this far into questions, this would simply be a duplication of the existing research council machinery, with all its expert committees and so on.

Second, this is precisely the pattern of research support in the United States. With the much greater number of people they can call upon for public service, the system of financing research by executive ministries has been quite successful in tactical detail, but so inadequate on a strategic level that they are moving to greater emphasis on a non-executive agency (NSF) and less project-oriented grants.

Third, scientists capable of something better than run of the mill work may be willing to accept a short term contract if it leaves them free to do what they want; but who is going to buy the Rothschild package which combines insecurity of tenure with official direction?

Thus even if the Rothschild scheme was put into full operation, it would have many disadvantages. Its real dangers are that it will be put into half-operation; the customer-contractor system may be set up before the customer ministries have any effective chief scientists, but only someone in administration who does not see why he cannot do the job well enough. The figures and dates given in Rothschild's Table 4 are presumably intended mainly as a provocative cock-shy, but if anything approaching those transferences were made with the haste suggested, there is a very real possibility that the ARC and NERC will be disrupted, to divert much of their funds straight down the drain.

The Dainton Report is at least less dangerous, but does not go far enough. It would bring Britain halfway to where the USA was in the Kennedy era; with a board of research councils coordinating their activity, rather as the US Federal

Council of Science and Technology (FCST) coordinated the American agencies, but still without anything corresponding to the President's Scientific Advisory Committee (PSAC), curtailed though its powers now are.

Finally, a comment which some people will consider frivolous but which is not. The real nature of our research councils is that the person in charge (secretary or what-have-you) has been enabled to operate like a renaissance Pope; his freedom is restricted only by a council and committees whose members he can select himself. It works wonderfully if the Pope is a Medici who likes having his ceilings decorated by Michelangelo. But there is an inherent tendency, in the world as it is, for an enlightened patron, ready to back his fancy for molecular biology in the late forties against the whole biological establishment, to be followed by someone less abrasive and penetrating.

One way of dealing with the situation is to have a fairly quick turnover of heads of councils, with many more people serving for quite short periods on the critical committees—this is what the United States does. We have tended to favour longer periods of service of quite a few people—a major research council may contain only one man in a large field like genetics, and he may stay there for ten years. Then it all depends, is he the right man or not? We have done quite well on the whole, but there is room for improvement. And the obvious step, in this sort of operation, is to have more, smaller, highly personalized research councils, who can pick and choose whom they want to support, and who can be picked and chosen by the scientists who are shopping for a patron.

So long as discussion remains confined to "either Rothschild or Dainton", many of the most attractive improvements will never get a hearing. For instance, what about leaving the research councils with essentially the same financing as at present (except for a few items which are clearly already on a customer/contractor basis), but giving the relevant ministries the chairmanship of some of the appropriate boards or committees which do the donkey work of the councils—the MRC Clinical Research Board, the ARC Animals Committee, and Plants and Soils Committee, and the similar specialist committees in NERC? It would be necessary to strengthen the scientific sections of the ministries to provide them with people who could even chair and make worthwhile contributions to such bodies; but it would provide a means of mutual education between the research and executive interests, and this is, I suggest, what we need, rather than dominance of either one over the other.

NEWS AND VIEWS

The Earth's Cores

THE inner core of the Earth has for too long lived in the shadow of the outer core. A great deal is known about the outer core: it is liquid, it has a radius of 3,480 km and it is almost certainly made mostly of molten iron. The inner core was detected because some seismic energy was recorded by seismographs lying between 105° and 143° from earthquakes. In the absence of an inner core these instruments should have lain in the shadow of the core and have received no energy. It seemed impossible that the energy was diffracted into the shadow and it was postulated that there was an inner core with a radius of 1,200 km, from the surface of which the seismic waves were reflected.

Having found or suspected that the Earth has an inner core it was natural to enquire whether it is solid or liquid. There was a certain convenience in having it solid, because it could then be supposed to be made of the same material as the outer core and to be solidified by pressure. This was an easier view to explain to undergraduates than the alternative that the two cores were composed of two immiscible liquids. Are there immiscible liquids at high pressures and temperatures? Maybe not, but who can tell? It is possible that a liquid can be formed from molten iron by collapsing the partially filled D-shell of extranuclear electrons; maybe it will not mix with ordinary molten iron. It would seem that the matter could easily be settled by showing that both compressional and distortional waves can travel through the inner core; to transmit distortional waves is an essential property of a solid. The distortional waves have not in fact been found, but the seismogram fanciers are not willing to say that they are absent. In these circumstances, those people not immediately concerned mostly concluded that very little was known about the inner core and that it was best left to the experts till its properties became clearer.

In this issue of *Nature* (page 465) Dziewonski and Gilbert provide strong evidence for the solidity of the inner core. The Earth possesses "normal modes" which are excited by earthquakes. To deduce the structure of the Earth from them is rather like deducing the structure of a piano from the noise it makes when thrown downstairs. It is possible because there are already rather good models of the Earth derived from seismic travel times. Dziewonski and Gilbert have picked out particular kinds of vibrations which have an appreciable part of their energy (up to 20 per cent) in the inner core and the frequency of which therefore depends markedly on its properties. The results are entirely compatible with a solid inner core and Dziewonski and Gilbert believe that no model with a liquid inner core can satisfy all the data.

Dziewonski and Gilbert's model is important. If the transition from the inner to the outer core is a transition from the solid to the liquid forms of a single material, then the boundary must be at the melting point and a constraint is put on the thermodynamics of the Earth's interior. That the boundary is at the melting point is an

essential assumption in an argument put forward by Higgins and Kennedy (*J. Geophys. Res.*, **76**, 1870; 1971) which suggests that the outer core is stably stratified. If this is so the large-scale steady motions usually postulated as constituting the dynamo that produces the Earth's magnetic field are presumably not possible (for a possible escape from this misfortune, see Bullard and Gubbins, *Nature*, **232**, 548; 1971).

The very existence of an iron core suggests ideas on the early history of the Earth. It is unlikely that the Earth accumulated in two stages, first particles of iron forming the core and then particles of silicate forming the mantle. Whatever the exact mechanism it seems likely that at the start the particles of iron and silicate were mixed up. If the accumulation were very rapid or if it took place in a very opaque atmosphere, the energy of impact would cause the particles to melt and could lead to separation of the core beneath the lighter mantle just as iron separates from slag in a blast furnace. Alternatively, if the rate of accumulation is slow the gravitational energy of each incoming particle will be radiated away and the Earth will accumulate as a solid with a temperature of a few hundred degrees. It is then possible that radioactivity will cause melting and separation of the core after a time of the order of 10^8 years. There are therefore two possible views about the history of the core; it could have formed as the Earth accumulated or its formation could have been a subsequent event. The second is the fashionable view, though it cannot be said that the arguments in its favour are conclusive.

On page 463 of this issue, Oversby and Ringwood argue for the first view—the very early formation of the core. They point out that the formation of the core would remove lead from the mantle and decrease the Pb/U ratio. The lead at present in the mantle is partly that left after the formation of the core and partly that produced by decay of uranium and thorium in the mantle. If the original isotopic composition of the lead incorporated in the Earth, the present composition of the lead in the mantle and the proportion of lead removed in forming the core are known, then, according to Oversby and Ringwood, the time of formation of the core can be found. These quantities can, with varying degrees of plausibility, be said to be known, and the result favours an early formation of the core.

Details of the argument will be awaited with interest. The formation of the core while the outer part of the Earth was solid would have been a cataclysmic event (there is a large release of energy); it must have occurred before any of the rocks now at the Earth's surface were formed. The ages of the oldest known rocks have been increasing and are now about 3,980 million years. The margin between this figure and the age of the Earth (4,550 million years) is becoming uncomfortably narrow and early core formation seems, on these grounds, desirable.

Cooperation between B and T Cells

THE experimental evidence which underlies theories for the function of B and T cells—lymphocytes which originate from the bone marrow and thymus respectively—derives from mice and latterly rats. Some support can be gleaned from various immunodeficiency syndromes in man but the picture presented is, as always with nature's uncontrolled experiments, rather confusing. Indeed, those immunologists who work with sheep find the whole notion of B and T cells unhelpful. But from all these different opinions may arise the heat to temper something solid which can actually be used in rational manipulations of immunological processes. On page 454 of this issue of *Nature*, H. W. Kreth and A. R. Williamson present a hypothesis for the mode of interaction of B and T cells which, if it can be substantiated, will present a valuable way of thinking of one of the associations between lymphoid cells.

If small numbers of spleen cells, from mice which have been immunized with DNP-BGG (dinitrophenol coupled to bovine gamma globulin), are transferred into irradiated syngeneic recipients and the antigen is injected again, production of anti-DNP antibody can be noted. The antibody produced in the original host is heterogeneous but in the secondary host the heterogeneity is sometimes much less. Furthermore, if spleen cells from the secondary host are transplanted into a third host (along with antigen) not only can the immune response be maintained but the exact quality of the antibody is reproduced.

This type of experiment has been said to lead to the production of clones of B cells capable of producing homogeneous antibody. Some reservation should be expressed about the "clones" because their identity is said to be established by their protein products rather than by, say, a cytogenetic marker. The assumption is here made that it is unlikely that cells of two different lineages should produce the same protein molecule; in other words, specific immunity is neither infectious nor polyphyletic. But aside from these considerations the capacity to produce the homogeneous anti-DNP antibody has been shown to depend on the activity of B cells. There has also been revealed a requirement for carrier-primed T cells. For example, if the spleen cells used to transfer the capacity to produce anti-DNP antibody are incubated, on transfer, with anti- θ antiserum (to eliminate T cells) in the presence of complement the immune response fails. An anti- θ depleted spleen cell population can be restored to its usual functional capacity by the addition of carrier primed syngeneic spleen cells. This type of experiment which implicates the T cell in relation to the anti-carrier and the B cell in relation to the anti-hapten response has led to the hypothesis that interaction between T and B cells involves an antigen bridge. The T cell is imagined to be at the carrier foundations of the bridge proffering the hapten in a particular manner to the B cell on the other side.

In earlier experiments it has been shown that attempts to elicit anti-DNP antibody formation, on transfer, by the use of DNP bound to a different carrier (for example, ovalbumin, OA) are unsuccessful. This suggests that the T cell, which is responsible for the anti-carrier response, makes a specific contribution to the immune process. Kreth and Williamson reasoned that this may not be so. They offer in evidence the fact that if spleen cells from

an animal producing a homogeneous anti-DNP antibody are transferred to an irradiated recipient along with DNP bound to a heterologous carrier (DNP-OA instead of DNP-BGG) a secondary anti-DNP antibody response results if unprimed AKR spleen cells are added to the inoculum. Kreth and Williamson argue that the reaction of the T cell component of the AKR spleen populations against the CBA (B) cells in the immune syngeneic spleen cell population provides the necessary bridges and non-specific stimulation which are required to promote anti-DNP antibody production in the presence of DNP bound to any carrier. With this experimental finding as the basis of their case they go on to adduce a general theory for interaction between B and T cells.

Kreth and Williamson suggest, following Burnet, that cells have a surveillance function in relation to other cells. B cells can firmly bind antigens to specific receptors on their surface but beyond that point they may need some further stimulus to react. Wandering T cells, finding the foreign material bound to the B cells, form some link with the altered B cell, and produce a mitogenic stimulus which triggers the further response of the B cell to the bound antigen. In their experimental studies Kreth and Williamson envisage that by providing an artificial difference between the interacting B and T cell populations they exaggerated the kind of response which normally occurs between B and T cells. They cite the evidence that T cells are responsible for effector function in cell-mediated immunity and they feel that their postulated role in B-T cooperation systems is consistent with this aggressive potentiality.

Perhaps the weakest point of Kreth and Williamson's theory is the lack of any suggestions about the "appropriate receptors" which are said to enable the surveying T cells to recognize foreignness on the surfaces of other cells. This failing, however, is not unique when the discussion turns to interactions between such complex matrices as mammalian plasma membranes.

Righting the Applecart

THE discovery during the past year of pulsating X-ray sources with little or no easily detectable radio and optical emission has upset the pulsar applecart. It has become obvious that some phenomenon other than the accustomed pulsar models is active in these energetic sources. For the present the questions are what sort of phenomenon, and is one extra phenomenon sufficient to account for the observations?

Henriksen, Feldman and Chau (Queen's University, Ontario) believe they have answered the first of these questions with their model, which they apply to Cen XR-3 on page 450 of this issue of *Nature*. The general applicability of the model has yet to be confirmed, but it could well turn out that the application of this model to other pulsed X-ray sources will answer the second question too.

Apart from the pulsations themselves, several of the newly observed sources are characterized by emission which can be interpreted in terms of black body radiation or by thermal bremsstrahlung. Henriksen, Feldman

and Chau favour a black body spectrum, and in fact the data on the recently discovered X-ray pulsar Cir XR-1 seem to be a fractionally better fit to the black body spectrum than to thermal bremsstrahlung. Henriksen, Feldman and Chau's work may also be relevant to the X-ray source GX 340+0 which has recently been determined to have in all probability a black body spectrum, although there is no evidence so far that GX 340+0 is pulsating. This is the only object so far for which the resolution is adequate to distinguish between the black body and thermal bremsstrahlung models.

The model by Henriksen, Feldman and Chau is based on the generation of heat in the crust of what they call a wobbling neutron star, and the periodicity of about 5 seconds that is observed in the X-ray data is attributed to the free nutation period. It seems too much to hope, however, that all the pulsating X-ray sources will fit neatly into this scheme. Although knowledge of the characteristics of pulsed X-ray sources is still in a confused state, two classes of sources are emerging. On the one hand are the sources Cen XR-3 and NP 0532 (the pulsar in the Crab Nebula which is detectable at frequencies from the gamma ray to the radio band), which are characterized by well-defined periodicities, implying that some phenomenon involving the whole star—such as rotation—is involved in the timing mechanism. On the other hand, however, are sources such as Cyg X-1 and Sco X-1 which seem to emit short trains of pulses whose periodicities can be different from one pulse train to the next. Some periodic phenomenon involving only the atmospheres of the objects seems the likeliest explanation that will allow this kind of irregularity.

Nor is this to say that further sources in the first class will be susceptible to the treatment of Henriksen, Feldman and Chau. For the present, other more established views based on the population of radio pulsars seem to give a good account of the X-ray emission from NP 0532, and such models would also presumably work for PSR 0833-45 if and when X-ray emission is detected from this other fast pulsar. For the time being, then, the deep pulses from Cen XR-3 containing 70 per cent of the radiated power (2–6 keV) seem to be in a class of their own.

GX3+1

Two Candidates

ALL but two possible candidates for the optical counterpart of the X-ray source GX3+1 have been eliminated by two successful lunar occultation experiments, which have reduced the uncertainty in its known position by a large factor. The Skylark rocket flights which have produced this improvement were carried out from Woomera for the University of Leicester group (on September 27 this year) and for the University College, London, group (on October 24). It was necessary to take advantage of the rare occultation of the source by the Moon (see *Nature Physical Science*, **233**, 106; and **234**, 2; 1971) because in the best error circle available previously there were a great many

stars, but none of them had a sufficiently striking spectrum to make it a likely candidate as an optical counterpart of the X-ray source.

Both groups have now completed the reduction of their data, and their results have been combined to give an improved fix on GX3+1. This fix is so good that the exact positions of faint stars in the area have had to be re-determined for comparison with the X-ray data. Two possible candidates have been found, lying one either side of the error box and close to RA 17h 44m 51.5s dec $-26^{\circ}33'50''$. Either GX3+1 is associated with one of these stars (numbers 14 and 15 in the notation of Kunkel *et al.*, *Astrophys. J. Lett.*, **161**, L169; 1970), or it is fainter than 18th magnitude and lies between them, or it is fainter than 21st magnitude and lies somewhere else in the error box.

Interaction between HL-A Antigen Determinants

HL-A ANTIGENS, like other genetically based polymorphisms in man, have acquired a mystique which has tended to isolate discussion to the habitués. The meaning of "HL" is itself a matter of some doubt. By usage it has come to be accepted as an abbreviation for "human leucocyte", but its propositors (see Amos, *Science*, **159**, 659; 1968) meant it to stand for "human histocompatibility locus".

It seems that HL-A antigens are controlled chiefly, but perhaps not exclusively, by two closely linked genetic loci often known as the first and the second. At each locus a number of alleles are recognized the activity of which results in the synthesis of various antigens designated (for the most part) HL-A1, HL-A2 and so on. Unfortunately, consecutive numbers can relate to antigens associated with different loci; for example, HL-A8 is determined by locus 2; HL-A9 is determined by locus 1. This muddle has arisen because the antigens and their controlling genes were not discovered in a nice neat sequence. The antigens are usually recognized by the use of specific cytotoxic or complement-fixing antisera used on leucocyte or platelet populations *in vitro*. A report by Legrand and Dausset in next Wednesday's *Nature New Biology* (December 29) reveals an interesting facet of these tests.

The leucocytes from a single human individual can carry four different antigens determined by the first and second loci, that is, when heterozygous at both loci. Legrand and Dausset took the cells from such an individual (HL-A3, 5/10,12; 3 and 10 determined by genes at the first locus, 5 and 12 by genes at the second locus), and incubated them with

a selected antibody (say anti-HL-A3) in the absence of complement. When the cells showed no further capacity for the absorption of anti-HL-A3 antibody they were incubated with one of the other three relevant antisera and the specific cytotoxic effect of the resulting supernatants was quantitated. By reference to the gene loci it can be said that sequential incubation with anti-HL-A3 and with then anti-HL-A10 is an allelic combination, anti-HL-A3 followed by anti-HL-A5 is a *cis* combination, and anti-HL-A3 followed by anti-HL-A12 is a *trans* combination. There are four of each kind of combination of different antisera. Legrand and Dausset found that when the antigens were attacked in the *cis* combination incubation with the first antiserum inhibited the absorption of the second antiserum. Further studies on genotyped patients established that this was a general rule.

This finding that there can be interference between combining sites on the cell surface which are determined by "adjacent" genes on a chromosome is in agreement with previous studies with mouse (H-2) histocompatibility antigens. The inference to the geneticist, as indicated by Legrand and Dausset, may be that two HL-A genes in the *cis* position cooperate in the building of a single molecule. To the transplanters it may well be that if an antibody to a particular antigen can be elicited which can block combination of antibody with another antigen (in the *cis* position), certain typing mismatches may be of no consequence. It would also be of interest to determine whether the inhibiting antibody prevented attack by cytotoxic cells.

THE ARCTIC

Peppering of Pingos

from our Geomagnetism Correspondent

ARCTIC seas are not, for obvious reasons, heavy shipping areas, with the result that bathymetry is poorly known and possible hazards to shipping are almost completely uncharted. The discovery of oil in Alaska and the use in 1969 of the SS Manhattan to test the feasibility of waterborne access to the oil fields have led, however, to an extensive programme for charting of northern seas and the investigation of the seafloor, chiefly by Canada. As a result of such studies in the Beaufort Sea, Shearer *et al.* (*Science*, **174**, 816; 1971) are now able to report their discovery of a feature of the ocean floor which is of both practical and academic interest—the pingo.

The road to the discovery was actually begun by an observation taken during the voyage of the Manhattan to Prudhoe Bay in 1969. At one point during passage through the Beaufort Sea (actually about 120 km north-west of Atkinson Point, Northwest Territories) there occurred a rapid shoaling which led to a reduction of water depth from 49 m to 23 m within a horizontal distance of 200 m followed by an equally abrupt return to the original depth. The region including this topographic feature has since been the subject of a more detailed bathymetric, sonar, gravimetric, magnetic, seismic and sampling survey which has led to the discovery that the original feature—nicknamed the “Admiral’s Finger”—is far from unique. The whole area is in fact peppered with similar underwater mounds rising from an otherwise smooth seafloor. Each such mound is usually irregularly and asymmetrically shaped with one side steeper than the other. The base diameter averages 400 m, the average elevation is 30 m, and in most cases a moat up to 10 m deep surrounds the base. Within the 5,000 km² survey area seventy-eight mounds were discovered scattered apparently at random and with depths of water above their summits ranging from 15.4 to more than 45 m.

In a region where shipping is already subject to the iceberg hazard, the existence of mounds scattered randomly around the seafloor is, of course, a matter of severe practical importance. Academic interest, on the other hand, centres on their cause. The mounds do not seem to correlate with any other of the geophysical parameters measured simultaneously with bathymetry. The magnetic field, though measured to the nearest gamma, indicated no anomalies in the sub-bottom structure; nor did the gravity field measured to the nearest milligal. Moreover, seismic studies showed the sub-bottom reflectors to be

more or less horizontal beneath the mounds.

What are these mounds? Basing their argument largely on a comparison between the morphology of the mounds and that of similar structures found 120 km to the south on the Tuktoyaktuk Peninsula, Shearer and his colleagues suggest that the features are pingos—hills which have a central core of ice. This being the presumed case, all that needs to be decided is when and how they were formed.

There seem to be two main possibilities—the terrestrial origin and the submarine origin. By the first the mounds are the submerged portions of pingos formed above sea level during the last glacial maximum when, as a consequence, the sea was about 100–150 km below its present level. The exposed portions of the present Beaufort Sea floor, outside the ice margin but subject to permafrost, would have contained lakes which did not freeze completely to the bottom. Some of these would subsequently have been shoaled either by infilling or draining to produce a body now defined as a pingo. The problem with this idea, however, is that it is difficult to imagine that subsequent wave action would not have eroded the pingos away by now.

The second possibility is that the mounds are pingos “in the genetic sense”—formed in the submarine en-

vironment since the postglacial rise in sea level. If the lakes in the previous example were not shoaled before the inundation by seawater, the remaining fresh lakewater would have to mix with seawater when such inundation took place. In this case the unfrozen water of the lake would come under the thermal influence of the overwhelming sea. Because the seawater temperature was almost certainly below -1°C —that is, lower than the temperature of the lakewater—the 0°C isotherm below the old lake would have moved downwards into the sediment thereby causing a freezing of the interstitial water and the consequent upheaval of the overlying material.

Of the two theories Shearer and his colleagues favour the second because of the erosion problem. They then go on to estimate the minimum age of a mature pingo by estimating the time required to freeze the depression of the permafrost layer formed in the insulated region beneath the original lake bed. Making reasonable assumptions about the size and shape of the depression, the thermal conductivity of the sediment, the surface temperature and geothermal heat flow, they come up with the figure of 5,000 years. In other words, on this estimate—and if pingos are of submarine origin—the postglacial rise in sea level must have taken place before 5,000 years ago.

A New Magnetic Reversal at 12,500 Years?

In next Monday’s *Nature Physical Science* (December 27) Mörner *et al.* report their discovery of a reversely magnetized sediment section about 12,500 years old. The section in question is part of an oriented core, 14.5 m long, from Gothenburg, south-west Sweden, which spans the time range 12,600 years to 8,600 years before present (from the early Ågård Interstadial to the mid-Boreal)—and there is little doubt that this dating is correct. The stratigraphy of southern Scandinavia is well known and a well dated climatic zone system has been determined for this range of time. For the Gothenburg core both climatic and stratigraphic dating agree well.

The core contains thirteen stratigraphic units, only the lowermost (earliest) sample (layer 13) of which is reversely magnetized. The boundary between units 13 and 12 is the boundary between the Ågård Interstadial and (above) the Fjärås Stadial and has previously been dated at 12,400 years old. Thus in the view of Mörner and his colleagues 12,400 years represents the upper boundary of a reversal event in the archaeological section of the Brunhes normal epoch.

The palaeomagnetic measurements, including magnetic cleaning up to 200

oersteds, show that the remanent magnetizations are stable—but normal for units 1–12 and reversed for unit 13. Mörner and his colleagues are thus well supported in their report that reversely magnetized material exists in the late Weichselian. But the contention that this represents a genuine field reversal is much less secure. For one thing no search for self-reversal characteristics is reported; and no confirmation from other sections is yet available. Moreover, although the probability that many short events remain to be discovered is now widely accepted, it is equally clear that the definition of short events is likely to be difficult—partly because a given short event will tend to be recorded less often, partly because such events are hard to distinguish from the spurious effects unavoidable in the Earth sciences, and partly because if self reversal operates at all it is likely to manifest itself exactly in this small scale situation.

The best known, and better documented, short reversed event alleged to lie in the Brunhes is, of course, the so-called Laschamp event; and this is far from being widely accepted as valid. Under these circumstances it may seem a little premature to name the new, apparent event.

PROTEINS

Enzyme and Means

from our Molecular Biology Correspondent

FRIEDEN's group in particular has helped to establish bovine glutamate dehydrogenase as an archetype for a ligand-controlled enzyme. Moreover, a great number of physical methods have been brought to bear on the problem of identifying the various enzymatic manifestations with physical changes in the protein, in respect particularly of conformation and association state. The enzyme possesses both glutamate and alanine dehydrogenase activities. The first is activated by ADP, which is also known to promote the well known propensity of the molecule to associate indefinitely to a highly polymerized form. GTP, on the other hand, tends to dissociate the enzyme, while inhibiting glutamate dehydrogenase and stimulating alanine dehydrogenase activity. The evidence has not favoured the original conjecture that the glutamate dehydrogenase activity was associated solely with the polymer, but the issue has only now been conclusively settled.

R. Cohen has devised a method of observing the sedimentation of an enzymatically active species in the ultracentrifuge in minute concentration. It involves the use of a substrate, which on exposure to the enzyme causes an adsorption band to appear or to vanish, according to whether the substrate or the product is the accessibly absorbing species. A layer of enzyme solution is allowed to sediment in a band-forming centrepiece through the substrate solution. The ins and outs of the technique are described by Cohen and Mire (*Eur. J. Biochem.*, **23**, 267; 1971), and it clearly has great advantages, including the low requirement in material, the possibility of observing the enzyme in question in a protein mixture, and of operating at protein concentrations so low as to be otherwise non-existent from the viewpoint of the ultracentrifuge. Diffusion coefficients can also be determined. In the accompanying article (*ibid.*, 276) Cohen and Mire show results for a series of enzymes, including some not yet isolated in pure state, and determine the sedimentation coefficients of the active oligomeric states. There are no surprises, and for glutamate dehydrogenase in particular both glutamate and alanine dehydrogenase activities sediment at about 13S, which corresponds to the unassociated hexamer, which, according to hydrodynamic analyses of the association equilibrium, is the expected state at such low concentrations.

Arnold and Maier (*Biochim. Biophys. Acta*, **251**, 133; 1971) have isolated, crystallized and characterized glutamate

dehydrogenase from rat liver. In terms of its sedimentation coefficient, and its reactivity towards antibodies directed to the bovine enzyme, it is indistinguishable from the latter. There is, however, no trace of the self-association behaviour. It seems by no means unlikely that this phenomenon is in fact a red herring so far as the physiological function of glutamate dehydrogenase is concerned.

Another issue which has been a matter of dispute for some time is whether glutamate dehydrogenase has one, or more than one, functionally important binding site per monomer for its coenzyme, NAD or NADH. The presence of two sites has now been inferred from circular dichroism measurements by Jallon and Iwatsubo (*Biochem. Biophys. Res. Commun.*, **45**, 964; 1971), closely followed by Koberstein and Sund (*FEBS Lett.*, **19**, 149; 1971). NADH, when it binds to the enzyme, greatly enhances optical activity, in the form of a sizable positive Cotton effect in its near-ultraviolet absorption band. In the presence of the modifying ligand, GTP, or of zinc ions, which exert the same kind of functional directing effect, this Cotton effect suffers an invasion and enhancement. A similar effect is brought about by the substrate, glutamate, in the presence of NADH. In the quaternary complex with substrate, NADH and zinc, however, the amplitude of the Cotton effect shows a further increase.

Observation of the ellipticity as a

function of the NADH concentration in the presence or absence of substrate and activator indicates that the cofactor does indeed attach at two sites. The first and stronger is associated with a Cotton effect, that is modified in sense and magnitude by GTP or zinc ions, both of which may well interact directly with the NADH, its binding site, or both at once. The diphosphate, NADPH, will bind at only one site. ADP, when added to the ternary complex, chases the second NADH from its site. The suggestion of Jallon and Iwatsubo is that the second NADH site is made manifest by a conformational change resulting from the binding of the effector ligand.

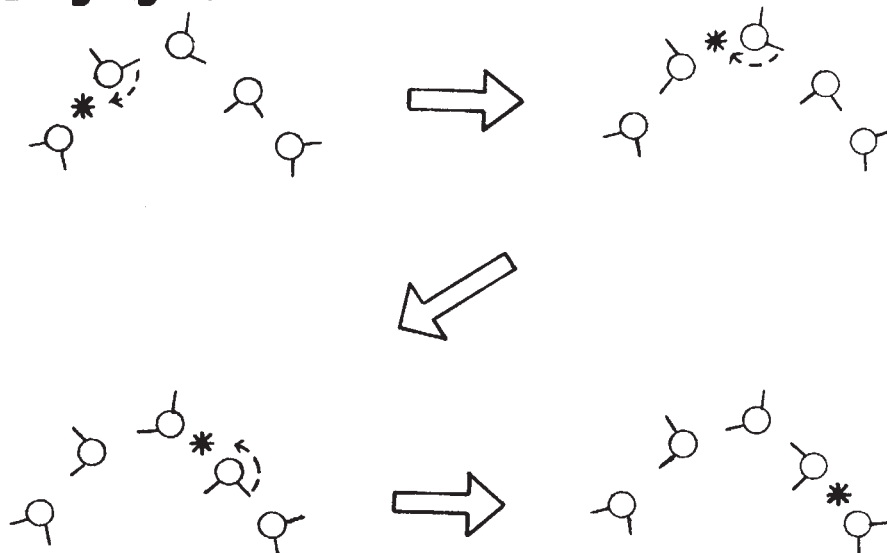
PHOTOBIOLOGY

Light and Life

from a Correspondent

THE use of damaging radiation on biologically active molecules was the central topic of a joint meeting on December 7 at the Royal Institution in London of the British Photobiology Society and the Society of Chemical Industry. It was clear from the remarks of Dr A. Knowles (University of Sussex) that low energy solar radiation not absorbed directly by a biological system can be very destructively utilized in a photodegradation reaction if an appropriate sensitizer is present. Under most conditions the triplet state

Dangling Bonds



IN next Monday's *Nature Physical Science*, Dr A. P. Hinton of the National Institutes of Health, Bethesda, presents computer calculations of the relaxation of the electric dipole moments of water in terms of a point defect model. The model is based on the fact that although the O-H...O bond is flexible there are some O-H groups

that do not participate in a hydrogen bond.

These dangling bonds are considered by Hinton as being analogous to a crystalline point defect. The figure here shows a schematic representation of the migration of such a dangling bond by means of sequential reorientation of water molecules.

of the sensitizer, which can be a dye molecule, is thought to interact with the substrate in one of several ways including direct energy transfer or by way of a second reactive species such as singlet oxygen. Dr Knowles indicated that his studies on the photoinactivation of nucleotides could be explained by a free radical hydrogen abstraction process which forms peroxides.

Whereas most research on photochemical mechanisms has involved the use of isotropic fluid solvents, there were two reports at the meeting that suggested the need for new studies using reaction media which more closely simulate natural conditions occurring *in vivo*. Dr M. Weese (Davy Faraday Laboratory, Royal Institution) told of her work on photoactivated electron transfer from chlorophyll *a* to quinone in a rigid lecithin matrix. The phospholipid solvent allows this reaction to be investigated under conditions close to that found in the chloroplast: high concentrations of reactant which are constrained in their diffusional motion. The kinetics of the electron transfer process indicate that the reaction occurs between pairs of nearest neighbour molecules and the speed of the reaction is quite sensitive to the shape of the quinone used. Another atypical reaction medium was discussed by Dr F. McCapra (University of Sussex) who indicated that the efficiency of chemiluminescence can be strikingly increased when the reaction is allowed to proceed in solvents which form micelles.

The general feeling of the participants was that a better understanding of these reactions could result in the selective photomodification of certain biological systems. The use of light energy to inactivate viruses or to destroy individual amino-acids of a protein was discussed. Professor F. R. Whatley (University of Oxford) pointed out that paraquat, a bipyridylum salt, only showed herbicidal activity on green plants in the presence of light. This may mean that incident radiation and an appropriate sensitizer can be used to limit the effective life of these growth regulating substances.

MESSENGER RNP

Informofers

from our Cell Biology Correspondent

ONE of the more notable contributions made by Russian molecular biologists has been the investigations of Georgiev and his associates into the state of messenger RNA and its precursors in the nuclei of eukaryotic cells and the transport of messenger from nucleus to cytoplasm. In 1968 (*J. Mol. Biol.*, **33**, 251; 1968) Samarina *et al.* presented evidence which indicated that the DNA-like

RNA in rat liver cell nuclei is complexed with protein particles to form a ribbon-like structure with a series of 200 Å, 30S ribonucleoprotein structures along its length; they proposed the name informofer for these 30S protein particles, anticipating that this protein would prove to be involved in the transport of segments of DNA-like RNA from the nucleus to the cytoplasm where it might act as messenger. Lukanidin, Aitkhodzina, Kulguskin and Georgiev (*FEBS Lett.*, **19**, 101; 1971) now report a simple procedure for the isolation of intact unaggregated informofer particles which are free of RNA.

By dialysing nuclear ribonucleoprotein aggregates against 2 M NaCl and then separating the products of dialysis by centrifugation through sucrose gradients Lukanidin *et al.* were able to isolate the free informofers. When exposed to urea each informofer dissociates into its subunit polypeptide chains, over 90 per cent of which are identical and have a molecular weight of about 40,000. Intact informofers readily interact *in vitro* with DNA-like RNA to reform the ribbon-like aggregates which, with a series of 30S informofers strung out along an RNA chain, superficially resemble polysomes.

With Williamson, of the Beatson Institute in Glasgow, Lukanidin and Georgiev (*ibid.*, 152) have compared the informofer protein with the protein to which the messenger RNA moiety of cytoplasmic ribosomes is associated.

Their aim was to discover whether or not informofer protein is transported across the nuclear envelope and into the cytoplasm with messenger RNA. To avoid the possibility of artefacts stemming from the leakage of informofer protein from the nucleus during the experimental manipulations they used reticulocyte polysomal ribonucleoprotein, for reticulocytes lack nuclei. Polyacrylamide gel electrophoretograms of informofer protein and the protein associated with globin messenger RNA reveal that the two classes of protein are very similar but definitely not identical. It seems therefore that informofer protein probably does not cross the nuclear envelope or, if it does, it is modified.

The disturbing possibility that the protein found associated with the messenger moiety of isolated polysomes is an adsorption artefact, generated as the polysomes are extracted, is shown to be highly unlikely by the recent experiments of Olsnes (*Europ. J. Biochem.*, **23**, 248; 1971). By a series of reconstruction experiments he has shown that artificial ribonucleoprotein complexes produced by incubating nuclear DNA-like RNA with cytoplasmic extracts are less stable than the messenger containing ribonucleoprotein complex isolated from polysomes; the artificial complex, for example, does not survive centrifugation through sucrose gradients. Olsnes concludes therefore that the polysomal, messenger ribonucleoprotein is probably not an artefact but exists *in vivo*.

Ribosomes and tRNA Recognition

It is often convenient to imagine the ribosome as a simple piece of cellular machinery the job of which is nothing more than to catalyse the formation of peptide bonds between a peptidyl-tRNA and an amino-acyl-tRNA which have reached juxtaposition as they decode a messenger. But that is a gross oversimplification of the ribosome's role in protein synthesis as investigations stemming from the discovery that the antibiotic streptomycin interferes with the codon-anticodon recognition process have amply proved. Indeed, Gorini suggests in next Wednesday's *Nature New Biology* (December 29) that the ribosome plays a considerable part in screening incoming transfer RNA molecules before and at the same time as they interact with messenger RNA.

Gorini has obtained strains of *Escherichia coli* which have mutated ribosomal proteins and has studied the effects of these mutations on the misreading of the genetic code and the suppression of nonsense codons by different species of suppressor tRNAs. The results of his experiments lead Gorini to suggest that there exists "a ribosomal screen which allows the bind-

ing of tRNA to the ribosome but antagonizes tRNA from reaching the stage of codon-anticodon interaction". The extent to which any particular species of tRNA participates in protein synthesis will as a result depend not only on the strength of the codon-anticodon interaction but also on the ease with which the tRNA can penetrate the ribosomal screen.

A complete understanding of precisely what happens during protein synthesis depends eventually, of course, on elucidating the three-dimensional structure of transfer RNA molecules and of ribosomes and messengers and finding out how mutations change the structure and function of these components. Little progress has so far been made with this structural analysis but as Kurland and Green report, also in next Wednesday's *Nature New Biology*, strains of *E. coli* with mutationally altered ribosomal proteins, which differ from their wild-type counterparts in the strength with which they interact with ribosomal RNA during the assembly of ribosomes, can be isolated by screening cells that have reverted from a state of dependence upon streptomycin.

Twenty-five Years of the South African CSIR

S. MEIRING NAUDÉ

Past-President, South African Council for Scientific and Industrial Research

In this article, Dr Naudé examines the development of the South African Council for Scientific and Industrial Research and highlights the underlying principles which guide it.

THE South African Council for Scientific and Industrial Research (CSIR) was inaugurated on October 4, 1945, and has thus been in full operation for more than twenty-five years. The two men primarily responsible for the establishment of this organization were the late Field-Marshal J. C. Smuts and Sir Basil Schonland—both Fellows of the Royal Society. Their vision, as embodied in the basic pattern of organization, has stood the test of time remarkably well.

Laboratories and Institutes

The president of the council is also the full-time chief executive officer, and is supported by a deputy president and three vice-presidents. The national research laboratories or institutes set up by the council, each with its own director, are similar in many respects to those of the former Department of Scientific and Industrial Research in the United Kingdom. A distinctive feature is that ten of these are located on one site on the outskirts of Pretoria, together with central technical and information services. There are three in Johannesburg and one in Port Elizabeth.

There are four industrial research institutes, which are patterned on the British research associations; a distinctive feature is that all four are associated with universities. Co-operative industrial research is also catered for by groups and units operating within the framework of the national laboratories and institutes but identified with, and supported by, the industries they serve. Contract research for private firms has been developed successfully by the national laboratories and institutes, though their chief sponsors are government departments and public bodies.

From the outset, the CSIR was given responsibility for "exploring and developing the whole field of medical research". This it did through a system of support for research units, groups and projects as well as *ad hoc* grants. This provided for the continuity of research, built up around individuals of outstanding ability, and avoided many of the problems of institutionalization. This activity, however, became the responsibility of the South African Medical Research Council which was established in 1969.

University research in other fields (excluding agriculture, geological survey and the human sciences) has been developed in much the same way. Funds provided for this purpose have been kept separate from the funds available to the research organization of the CSIR itself, and by drawing academic staff into the procedures for evaluating applications and adjudicating awards, they have shared in the invidious task of allocating funds which are inevitably less than the demand. By these means, justice has been seen to be done.

There has of course been the inevitable disagreement about the role of the national laboratories and institutes in the field of basic research. This has not deflected the

CSIR from its policy that the continued health of its own research requires a balance between applied and basic research. The council is assisted in maintaining the quality of research through a system of project steering committees, advisory committees and specialist advisers. Relevance of research is achieved by insisting that a project should, where possible, have a potential customer and by allowing projects to grow in relation to the extent of support or demand for sponsored work. Clearly there can be no hard and fast rule about this, but in practice it turns out that approximately 50 per cent of the revenue is derived from sponsored work.

This figure is somewhat misleading because two institutes, the National Institute for Road Research and the National Institute for Defence Research, are fully supported by the government departments concerned and not by direct Parliamentary appropriation. The particular merit of this arrangement is that, although the budgets of these institutes are controlled by these departments in terms of, for example, relevance of research programmes in relation to practical results, the research is directed by the CSIR. To a lesser extent, important programmes or projects in other institutes are sponsored in the same way by industries (on a cooperative basis) or by public bodies.

In retrospect it is a source of some satisfaction to find that the policy of establishing small centres of excellence within the national laboratories—either in new fields of research or in relation to particular fields of application—has paid off even better than we might have hoped. Two examples will suffice by way of illustration.

Immediately after the war Dr Schonland collected a small group of the brightest young men who had been associated with him in the field of radar and set them to work on the applications of radar to the study of atmospheric phenomena. He called this the Telecommunications Research Laboratory and set it up in the Department of Electrical Engineering of the University of the Witwatersrand.

One of the challenges presented to this laboratory was posed by Colonel Harry Baumann, then director of trigonometrical survey. He had been greatly impressed with the wartime developments in radar ranging and was convinced that there was tremendous scope for instruments which could measure distance with the accuracy required for geodetic and engineering survey purposes; but he recognized that a breakthrough in techniques was necessary because of the need for an increase in accuracy or about two orders of magnitude.

The response from the CSIR was typical. Although no success with such radio systems had been achieved elsewhere, I accepted the view of senior laboratory staff that there were no fundamental obstacles and authorized the work to proceed—even though the financial support being sought by Colonel Baumann had not yet been obtained. Such support never, in fact, materialized.

The work was entrusted to Dr Trevor Wadley, fresh from his remarkable success in the development of crystal controlled communication receivers. In a matter of months Wadley was able to demonstrate a working system with the accuracy necessary for geodetic purposes. This was the origin of the now well known Tellurometer system of distance measurement. With patents in the principal countries of the world, the CSIR was able to follow through with

development and commercialization through the establishment of Tellurometer Ltd, in association with a Cape Town manufacturer, and establish a world-wide market for the Tellurometer system. Much of the credit for this must go to the initiative of Colonel J. H. Jeffrey, a Cape Town entrepreneur. Tellurometer Ltd was subsequently taken over by a more broadly based company, the Instrument Manufacturing Corporation, which, in turn, was taken over by Plessey. In the meantime, the laboratory's work on distance measurement has led to improvements in the Tellurometer system and recently to a new infrared system which is more suitable for close range work.

Underlying Principles

The case history of the Tellurometer is presented here in some detail (if somewhat sketchily) because it does illustrate a number of principles. (1) The project was included in the programme because there was an established need and a potential sponsor; (2) work was initiated because one of the staff was interested and was allowed to continue because the initial results showed promise; (3) technical and theoretical back-up in the research group contributed much to effective follow-through; (4) development and commercialization were successful because the CSIR was prepared to entrust it to an enterprising individual; (5) experience with the invention provided much of the "entrepreneurial know-how" of the South African Inventions Development Corporation—a statutory body—which was subsequently set up in close association with the CSIR to handle the patenting and development of inventions; (6) there were many occasions when crucial decisions had to be taken quickly and decisively and followed up by resolute action.

The Telecommunications Research Laboratory has since developed into the National Institute for Telecommunications Research, with its own building on the campus of the University of the Witwatersrand; the first director, Dr F. J. Hewitt, is now a vice-president of the CSIR. In spite of the name, its activities could be more accurately described as radio physics, for example radar measurement of rainfall and the application of radar to the study of lightning flash.

Some people may ask whether this research is not more appropriate to a university than to a national research institute. The CSIR has, however, an obligation to provide scientific services to meet the engineering requirements of industry and the technical agencies of government—in this case, for example, the South African Broadcasting Corporation, the General Post Office, the South African Airways and the armed services. This requires regular measurement, recording and interpretation of data connected with the environmental conditions affecting the propagation of radio waves and the provision of regular services to these agencies. This type of activity is obviously not appropriate to a university. But, at the same time, these phenomena are not completely understood and it is therefore essential that the service activity should have a broad base of fundamental research—subject to the condition that all projects should be relevant to the interests of actual or potential sponsors. Not only does this serve to attract and retain staff of suitable calibre but it also enables the national laboratory to provide a "window" to worldwide advances in the field.

Clearly the national laboratory will not be able to maintain this position if the standards of the services provided by the technical agencies concerned, and the manufacturing industry supplying equipment to these services, are not keeping pace with developments throughout the world. This requires scientists, engineers and technicians who are well grounded in the techniques involved, and who work against a background of current research. In this respect, the CSIR has a central role in the support of academic research. Its support of university research includes support for the research activities of academic staff—because research students

will clearly not be attracted unless there are active research schools in the fields concerned.

In radio physics (or solar and terrestrial physics), for example, the CSIR supports ionospheric research and radio astronomy at Rhodes University, magnetospheric physics (micropulsations and whistlers) at the University of Natal, cosmic rays at Potchefstroom University—and, in the context of Antarctic research, it funds studies of geomagnetism through Potchefstroom University and of airglow through the University of Stellenbosch. The CSIR is also the national member of the International Council of Scientific Unions and thus of the International Union of Geodesy and Geophysics, the Union Radio Scientifique Internationale, the Committee on Space Research and the Scientific Committee on Antarctic Research. Such research (including that of the CSIR) is reviewed by the South African National Committee for Geomagnetism, Aeronomy and Space Sciences. This committee reviews South African participation in international programmes and activities initiated or coordinated at the international level by the International Council of Scientific Unions and provides a medium through which all who are interested in this field of science are kept in touch with each other.

A side-effect of this acceptance of responsibility in relation to global programmes was that, during the International Geophysical Year 1957–58, the CSIR undertook to operate optical and radio tracking stations for artificial Earth satellites on behalf of the United States national committee. Although South Africa had no part in these programmes, this was accepted as an international obligation because of South Africa's geographical position, which offered the first opportunity for interrogation of the satellites—twenty minutes after launching. Operation of the Minitrack (radio) Station by the National Institute for Telecommunications Research was so successful that the CSIR was later invited to operate a deep space instrumentation facility and a satellite tracking and data acquisition facility by NASA. The operation of these stations represents an enormous engineering exercise which is more appropriate to a national institute than to a university.

Sewage and Effluent

The National Institute for Telecommunications Research has been selected as an example to illustrate some aspects of the policies which have been evolved by the CSIR in fulfilment of the role and functions envisaged for it by its founder president, Sir Basil Schonland. Some other aspects may be brought out more clearly by considering the example of water research.

In the early days of the CSIR, the need for research into the treatment of sewage and factory effluent was brought to the attention of the CSIR by municipal chemists and their professional association, the Institute of Sewage Purification. At that time, two government agencies were concerned with water—the Department of Irrigation, which was traditionally concerned chiefly with water supply for agriculture, and the Department of Forestry, which had a well conceived programme of hydrological research into "forest influences". This was designed *inter alia* to provide data on the effects of afforestation on water supply. Thus the CSIR had to consider its position in relation to current research—or the lack of it—in hydrology and the foreseeable problems of water pollution. The last of these was a "horizon problem"—visible principally to the municipal chemists who were confronted with the problems that accompanied increasing industrialization and urbanization.

Confronted with this problem, which did not fit into any of the organizational patterns envisaged at that time, the CSIR decided on a tentative approach. It established a Water Treatment Research Unit in temporary accommodation at the University of the Witwatersrand and placed it

under the direction of the National Chemical Research Laboratory. The decision to set it up in Johannesburg was coincidental to the extent that the chief city biochemist of Johannesburg, Mr H. M. Wilson, was at that time available, on retirement, to take charge of the unit. To lead the research, the CSIR recruited Dr G. J. Stander, the city biochemist of East London and a hot gospel for the doctrine of water re-use as the only long term solution to the problem of water supply which would confront the country in the second half of the twentieth century.

The procedure adopted provides some interesting contrasts with that adopted in the case of radio physics. There was expertise available in radio physics, which was looking for applications in the transition from war to peace but, as far as water was concerned, there was just a "horizon problem", with no clear-cut lines of official responsibility for finding a solution. The CSIR therefore recruited the best available technological expertise within the framework of an institute under strictly academic and professional scientific direction.

The "tentative" approach of the CSIR paid off to the extent that, in response to the demand for the services of the Water Treatment Research Unit, it grew to the status of a division of the National Chemical Research Laboratory and, in 1957, to the National Institute for Water Research. Dr Stander is now the president of the International Association for Water Pollution and has left the CSIR to become deputy chairman and executive officer of a newly created statutory body, the Water Research Commission. And, for the record, the first municipal plant for the purification and recirculation of municipal sewage effluent in the world was commissioned at Windhoek in 1969 by the Prime Minister, Mr B. J. Vorster.

Methods of Organization

The two examples illustrate the principle that research cannot be organized from above. The policy of the CSIR has always been "to let the currency float"—in terms of current needs and available expertise and ability. At all times, staff have been required to prove their worth in terms both of the demand for their services from sponsors and of international recognition.

The principles deduced from these two examples illustrate most aspects of CSIR policy. For example, the National Physical Research Laboratory, which was established in 1946 and of which I was the first director, has provided the seed bed from which has grown the National Mechanical Engineering Research Institute, the National Research Institute for Mathematical Sciences and the National Electrical Engineering Research Institute. These research groups also provided the core from which was drawn the nucleus of staff of the Atomic Energy Board when the time was ripe for it to set up its own research establishment in 1959—under Dr A. J. A. Roux, formerly head of the Functional Efficiency Division of the National Building Research Institute, director of the National Mechanical Research Institute and vice-president of the CSIR.

Similarly, the National Chemical Research Laboratory was the nursery from which has grown the National Institute for Water Research, the National Nutrition Research Institute (recently split into the National Food Research Institute under the CSIR and the Nutritional Diseases Research Institute under the South African Medical Research Council), the Chemical Engineering Group, the Bantu Beer Research Unit, the Microbiology Research Unit (which has made particularly valuable contributions in the field of mycotoxins) and the Corrosion Research Unit.

Building Research

The third national research institute to be set up by the CSIR under Dr Schonland was the National Building

Research Institute. The reasons advanced at the time for setting this up as a national institute and not as an industrial research association were that, in view of the horizontal organization of the building and construction industry, the prospects of gaining support for an adequate research effort in this field on an equal basis were unlikely. This virtually enunciated a principle which has since become part of CSIR policy towards industrial research, namely that in industrial sectors where a need for research has been identified, it is incumbent on the CSIR to initiate research under the umbrella of an existing institute and to build it up with industrial support.

In 1945 and 1946, the most urgent need was for low cost, high density urban housing for the Bantu population. Wartime conditions had resulted in the crowding of the rural population into the cities and, with the prospect of burgeoning industrial development, there was every indication that the problem would become more acute.

Under Mr E. W. Dohse, chief engineer of the Department of Public Works, who was seconded to direct this institute, the problem was solved by a comprehensive programme involving all sections of the community. The principles of design and construction which were evolved have since been applied by many developing countries in Africa, South East Asia and South America. The institute has subsequently moved on to make important contributions to community services such as the design of schools, hospitals and homes for the aged and to specific problems such as the erection of buildings on expanding clay foundations and the utilization of blast-furnace slag with a high magnesia content for the production of slag cement.

The chief contribution of this institute, however, may well have been in the establishment of a Building Research Advisory Committee, which has brought together leading representatives of all sections of the building and construction industries. Quarterly meetings of this committee have provided a neutral forum for the discussion of technical issues affecting all sections of the professions, industries and trades between which there was previously no constructive dialogue. This institute has provided the home for the development of a Timber Research Unit and a Ceramics Research Unit—on the grounds that the principal industrial support was forthcoming for the use of these products in the building and construction industries.

In the field of housing, the CSIR became involved with human problems. But it had to face up to these from the start because of its obligation to "explore and develop the whole field of medical research" and by a special request to advise on the future of the Aptitude Tests Section of the wartime South African Air Force. This service, under Dr Simon Biesheuvel, had achieved great distinction during the war in the classification and selection of aircrew personnel. The CSIR decided, at its first meeting, to provide a "home" for its records, through the creation of a National Bureau for Personnel Research. Within the framework of the CSIR's policy for contract research, this soon developed into the National Institute for Personnel Research under Dr Biesheuvel's direction, which made its mark in the development of procedures for the selection, classification and training of workers—first in the mining industry but later in other fields of commerce and industry. This unique development is, to some extent, a reflexion of the CSIR's policy of allowing research to find its own level in response to demand for research services, the need for these particular services in a situation of rapid industrial development in a largely rural community (to a great extent primitive and tribal) and the particular abilities of the research leader. With the establishment of a separate Human Sciences Research Council, the future control of this research is under review, but there is no doubt that those aspects which are directly concerned with personnel management are inseparable from industrial research.

Fatty Acids and Hydrocarbons in Surtsey Sediment

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Hydrocarbons and fatty acids were examined from the sediments of a marine lagoon on Surtsey, an island formed by a volcanic eruption in 1963. The distribution of both hydrocarbons and fatty acids indicated that correlations can be made between the primary organisms observed in the lagoon, Chlorophyta, and the sediment components identified.

In 1963, a submarine eruption began approximately 20 miles off the south coast of Iceland, which gave rise to the island Surtsey. There was a gradual decrease in volcanic activity in 1964 and 1965, ceasing entirely at the end of April 1965. In the spring of 1964 the first biological observations were made. Since that time, colonization of plants and animals has been routinely surveyed. Growth conditions for newly arriving plant parts have been poor because the substrate is composed primarily of lava, tephra or secondary beach substrata¹. Of two lagoons formed, the older, which formed on the northern side of Surtsey, was studied in detail. In May 1965, filamentous Chlorophyceae were observed on the shore east of the lagoon and by 1967 the lagoon was populated by gulls. Large faecal deposits led to heavy microbial development. Just beneath the water's edge, brown layers developed in which large numbers of unicellular chlorophycean algae and rare flagellates² could be seen. The lagoon gradually dried up and by June 1970 only a brown-caked basin remained³. The sample used in this analysis was collected from the upper 6 cm of the surface of the dry lagoon and stored in a polyethylene bag. The sediment was a fine-grained, green mud with no apparent mat formation.

Biological compounds and their degradation products are found in trace amounts in sediments. It has been shown that a direct correlation can be made between lipids originating from biological sources and those found in geological sources^{4,5}. Although each new species of bacteria, plant, and animal

has been carefully recorded by biologists, no organic geochemical studies have been made on sediments. The only exception to this was the organic geochemical investigation of the amino-acids and hydrocarbons from the freshly fallen ash from an eruption resulting in the formation of the island, Syrtlinger, which occurred less than 1 mile from Surtsey. Ponnampuruma⁶ has reported the presence of alanine and aspartic acid, and traces of glycine and serine at a total concentration of only 10 p.p.b. in the freshly fallen ash of Syrtlinger; no hydrocarbons were detected. Further studies of the ash amino-acids have been carried out by Fridriksson⁷. The island of Surtsey presents a unique source for organic geochemical study⁸ because the organisms which contribute to the Surtsey sediments are known.

The sample was treated with dilute hydrochloric acid to remove carbonate and washed with distilled water to remove soluble inorganic salts. After filtration, the sample was subjected to four successive 30 min chloroform extractions with ultrasonic vibrations. The sample was filtered after each extraction and the extract taken to dryness on a rotary evaporator. The lipid residue was weighed and then saponified using 0.5 N potassium hydroxide in methanol for 2 h with gentle refluxing. The nonsaponifiable fraction, containing the hydrocarbons, was separated from the saponifiable fatty acid fraction by extraction of the alkaline solution with benzene. The separated nonsaponifiable fraction was then dried under a stream of dry nitrogen and taken up in 1 ml. of hexane. This was placed on a chromatographic column packed with prewashed silica gel and silicic acid (1:1, v/v) (Woelm, activity 1). The aliphatic hydrocarbons were eluted with 100 ml. of hexane and the hexane fraction dried on a rotary evaporator at 60° C. This hydrocarbon fraction was weighed and used for identification by means of gas-liquid chromatography. The saponifiable alkaline fraction was acidified with hydrochloric acid to pH 3 and extracted with benzene. The fatty acid-benzene fraction was taken to dryness. Methylation of the free fatty acids was accomplished by using BF₃-MeOH reagent. The fatty acid methyl ester fraction was weighed and used for identification by means of gas-liquid chromatography. The filtered sediment was then treated with 0.5 N potassium hydroxide in methanol and subjected to ultrasonic vibrations for 30 min. The sample was filtered and the alkaline solution was extracted with benzene. This benzene fraction was taken to dryness,

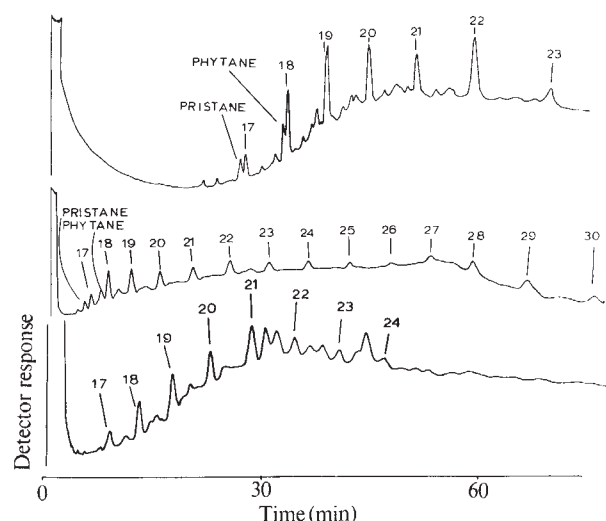


Fig. 1 Gas chromatogram of alkanes from Surtsey sediment (from lipid extracts). Top, stainless steel capillary column (150 feet $\times$ 0.02 inches internal diameter) coated with Apiezon L. Barber Colman (series 5000) gas chromatograph with helium flow of 9 ml./min (using split). Temperature programmed from 150° C to 230° C at 3° C/min. Middle, copper column (8 feet $\times$ 1/8 inches outer diameter) packed with 5% FFAP on Varaport 30. Barber Colman (series 5000) gas chromatograph with helium flow of 30 ml./min. Temperature programmed from 150° C to 225° C at 3° C/min. Bottom, gas chromatogram of alkanes from Surtsey sediment (from KOH-MeOH extraction). Barber Colman (series 5000) gas chromatograph with helium flow of 35 ml./min. Temperature programmed from 150° C to 230° C at 3° C/min.

weighed, and the hydrocarbons fractionated by silica gel/silicic acid column chromatography. The hydrocarbons were identified primarily by gas-liquid chromatography.

Table 1 Analytical Analysis of Surtsey Island Sediments

Dry weight sediment	1,419.2 g
Lipid extract dry weight	0.0192 g
Hydrocarbon dry weight	0.0036 g
Fatty acid methyl esters dry weight	0.0094 g
Exhaustive KOH-MeOH extract hydrocarbon dry weight	0.0009 g
Largest hydrocarbon component	C22 : 0
Largest fatty acid component	C18 : 1
Percentage lipid	0.0013%
Sediment weight	
Percentage hydrocarbon	0.00025%
Sediment weight	
Percentage fatty acid ester	0.00066%
Sediment weight	
Percentage hydrocarbon	18.75%
Lipid weight	
Percentage fatty acid ester	48.95%
Lipid weight	

A gas chromatograph (Barber Colman, series 5000) with flame ionization detectors, operated using temperature programming, was used for identification. Retention times in gas-liquid chromatography are not unique, so we verified the identifications by using two different columns, Apiezon L and FFAP (Varian), a non-polar and polar substrate respectively. Stainless steel, wall-coated open tubular columns, 150 feet $\times$ 0.02 inches (internal diameter), and copper columns, 8 feet $\times$

1/8 inches (internal diameter), were used for separations. Analytical results are shown in Tables 1 and 2. Fig. 1 contains chromatograms showing total hydrocarbon fractions. The hydrocarbons obtained from saponification were injected onto a 6 foot 1% SE-30 column of an LKB-9000 gas chromatograph-mass spectrometer and programmed from 130°–260° at 5° C/min. The mass spectra were scanned with a total ionizing current of 20.0 eV.

Table 2 Percentage Composition of Hydrocarbon Components from Chloroform Extracts

Hydrocarbons	Percentage composition
<i>n</i> -16	Trace
Pristane	4.2
<i>n</i> -17	7.0
Phytane	7.6
<i>n</i> -18	14.0
<i>n</i> -19	15.3
<i>n</i> -20	9.5
<i>n</i> -21	6.4
<i>n</i> -22	7.7
<i>n</i> -23	4.9
<i>n</i> -24	6.2
<i>n</i> -25	2.3
<i>n</i> -26	2.3
<i>n</i> -27	4.1
<i>n</i> -28	2.1
<i>n</i> -29	3.0
<i>n</i> -30	1.9
<i>n</i> -31	1.5

Fig. 2A shows the mass spectrum of the first component, which has a molecular ion at m/e 240, corresponding to normal C₁₇, and prominent fragment ions at m/e 113, 183 and 253 corresponding to pristane. Fig. 2B shows the mass spectrum of normal C₁₉ saturated hydrocarbon. The slight enhancement of the ion at m/e 225 may indicate some contribution of the *iso* C₁₉ alkane, 18-methylnonadecane. Fig. 2C gives a mass spectrum indicative of a mixture containing C₂₁ hydrocarbons with one and two degrees of unsaturation.

Table 3 Percentage Composition of Fatty Acid Esters

Fatty acid ester	Percentage composition
12 : 0	1.45
14 : 0	10.51
16 : 0	28.09
16 : 1	7.26
18 : 0	18.16
18 : 1	37.88
Others	1.21

Other mass spectra indicate that the hydrocarbons of the sediment may be far more complex than the gas chromatogram indicates. There are indications of additional straight-chain hydrocarbons ranging from C₂₃–C₃₆ with zero to three degrees of unsaturation. There were also traces of what may correspond to C₂₅–C₂₇ aromatic hydrocarbons. No measurable hydrocarbons or fatty acids were found in the procedure using controls and solvent blanks.

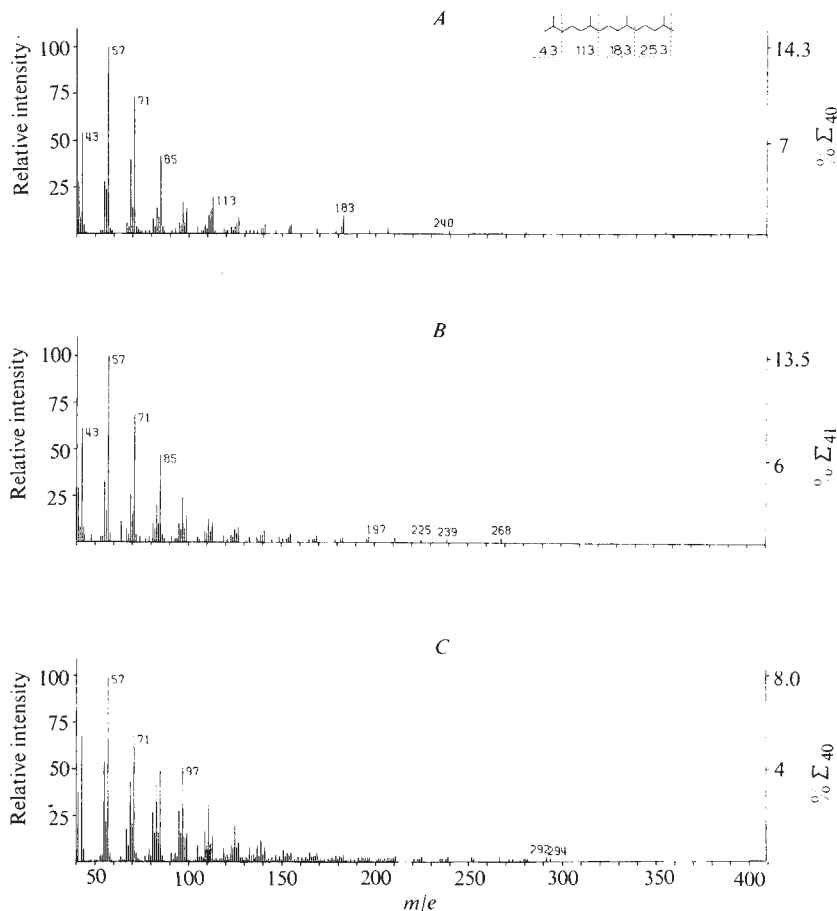
Hydrocarbons are present in recent sediments, usually in concentrations of parts per million (p.p.m.) and seldom exceeding one part per thousand (p.p.t.). The Surtsey sediment sample contained a hydrocarbon concentration of approxi-

mately 2 p.p.m., the low value due to low productivity of the island. Straight-chain hydrocarbons ranging from C_{17} to C_{25} were present in the Surtsey sediment, C_{22} being the largest normal hydrocarbon component. Pristane (2,6,10,14-tetramethylpentadecane) and phytane (2,6,10,14-tetramethylhexadecane), isoprenoid hydrocarbons, which probably arise from diagenetic degradation of algal chlorophylls, were present in trace amounts. But these isoprenoids do occur in some species of benthic algae⁹ and in some bacteria^{10,11}. Whereas higher plant alkanes show a marked trend towards odd-numbered carbon atoms between C_{25} and C_{33} , the Surtsey sediment alkanes show no marked odd/even tendency, as would be expected from a sediment which had no contributions from higher plants.

C_{21} – C_{24} molecular weight region in the chromatogram of the KOH-MeOH extracted hydrocarbons, which were absent in the chloroform extracted hydrocarbon fraction as shown (see Fig. 1). Also the higher molecular weight hydrocarbons (C_{25} – C_{30}) were absent in the KOH-MeOH extracted sample. As noted, the chromatograms of the exhaustive KOH-MeOH and the chloroform extracted hydrocarbons were not identical. This indicates that there is complex organic matter not extractable by lipid solvents alone.

Fatty acids are major lipid components of all living organisms which normally show a predominance of even-carbon numbered fatty acids. Algae contain a narrow range of fatty acids generally ranging from $C_{14}:0$ to $C_{18}:4$ (refs. 5, 17, 18, 19). Polyunsaturated fatty acids are absent in bacterial lipids;

Fig. 2 A, Mass spectrum of pristane and normal C_{17} hydrocarbon; B, mass spectrum of normal C_{19} hydrocarbon; C, mass spectrum of mono and diunsaturated C_{21} hydrocarbon.



If blue-green algae were the principal invader of the lagoon studied, one would expect to find a precursor relationship between the hydrocarbons found in blue-green algae and the hydrocarbons found in the sediment. This was not observed. Some bacteria, however, exhibit hydrocarbon distributions similar to that found in the Surtsey sediment^{10,15}. Chlorophycophycean algae (green algae) exhibit a more complex hydrocarbon distribution than Cyanophycophyceae (blue-green algae). Hydrocarbons from several species of *Chlorella*¹⁶ grown autotrophically under fluorescent light contained a series of saturated n-paraffins ranging from 17 to 36 carbon atoms in length. Among the algae found on Surtsey, Chlorophycophyta was the group most frequently found² and the hydrocarbons found in the sediment contained corresponding carbon chain lengths.

The hydrocarbons extracted from the sediment with chloroform and those from the exhaustive KOH-MeOH extraction were qualitatively the same in the lower molecular weight range (Fig. 1). But there were large, unidentified peaks in the

the mono-unsaturated acid, 11-*cis*-octadecenoic acid, is the largest component in photosynthetic bacteria. In contrast, 9-*cis*-octadecenoic acid is the major monoenoic acid found in blue-green algae. In general, blue-greens usually have 16:0 or 16:1 as their major fatty acid components while green algae have an unsaturated C_{18} as their major fatty acid¹⁷. The fatty acids (both free and bound) from the Surtsey sediment consisted of three major components, a straight-chain $C_{14}:0$, a straight-chain $C_{16}:0$, and a straight-chain $C_{18}:1$ fatty acid (Table 3). Other minor components identified by their retention times were $C_{12}:0$, $C_{16}:1$, and $C_{18}:2$ fatty acids. Further characterization of the C_{18} monoenoic acid found in the Surtsey sediment might further define the course of this acid.

Knowing the distributions and concentrations of both acids and hydrocarbons from the Surtsey sediment, however, it seems that green algae were the primary contributors of organic matter to the lagoon during its short existence, although bacteria may have made some contribution. Blue-green

algae are considered to be one of the more primitive organisms and the fact that this lagoon was observed to be inhabited primarily by green algae during the few years of its existence is of interest as well as the fact that the record of their brief inhabitation has been chemically preserved in the lagoon sediment.

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Cen XR-3 : A Neutron Star Younger than the Crab ?

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A model for the pulsed X-ray source Cen XR-3 has been constructed, based on studies of wobbling, young, evolving neutron stars which are generating heat in their crusts by the conversion of rotational energy through precession-induced cyclic strain. The observational features can be understood if the observed period is interpreted as the free nutation period. We predict the actual rotational period to be ~ 8 ms and the slow-down rate $\sim 8 \times 10^{-13}$ s s⁻¹.

REMARKABLE variations in the X-ray flux from Cen XR-3 have been reported by Giacconi *et al.*¹. These have the following characteristics: (1) There is an observed periodicity at ~ 4.87 s with a large duty cycle, the available data cannot exclude the presence of additional periods shorter than ~ 0.096 s; (2) the intensity was observed to increase ("flare") by a factor of

10 in about 1 h, and in the same 24 h interval the period was seen first to decrease and then to increase suddenly by 0.02% and 0.04%, respectively, each change occurring in $\sim 10^3$ s; (3) the observed period has shown a secular decrease of $\sim 0.6\%$ in a 3 month interval; (4) the "flare" X-ray spectrum observed in April 1971 can be fitted with a black-body curve corresponding to a temperature $\sim 3.3 \times 10^7$ K (or a thermal bremsstrahlung law with $kT \sim 16$ keV and an exponential cut-off at ~ 4 keV).

Wobbling Neutron Star Model

We wish to show in this article how most of these striking properties can be understood in the light of a recent study of wobbling, young, evolving neutron stars^{2,3} and in terms of our recently developed theory of heating in these objects¹⁶. This theory contends that rotational energy can be converted into heat in neutron-star crust by the action of precession-induced cyclic strain in the crystalline mantle and/or by core-mantle coupling (although this is expected to be less important)⁴. We have already considered Cen XR-3 as a possible application of this theory of neutron-star heating, and we have accordingly predicted its rotation period to be $\lesssim 10$ ms. We show here that this prediction can be reconciled with the new observations by supposing that the

period which has been observed by the ASE group¹ corresponds to the free nutation period of a triaxial neutron star.

It is perhaps not immediately obvious how such a periodicity might show itself in pulsed black-body emission. We therefore refer to the configuration of Fig. 1 where we have presented a simplified picture of the rigid body motion. The total angular velocity is shown as the vector ω , fixed in space to a high degree of approximation. It is taken to be at an angle θ with respect to the 3 axis, the axis of maximum moment of inertia. The observer's line of sight makes an angle i with the rotation axis so that the great circle $\overline{OH}$ represents the observer's horizon on the neutron star's surface. The axis m represents the radius vector to an assumed hot spot, which is likely to lie on the magnetic axis. The intersections of the dashed planes (passing through the hot spot perpendicularly to ω) with the stellar surface are the effective emission bands seen by an observer who cannot time-resolve on the scale of $2\pi/\omega$. As the axis m nutates ($\theta = \theta(t)$) from position 1 to position 2 through even a small angle

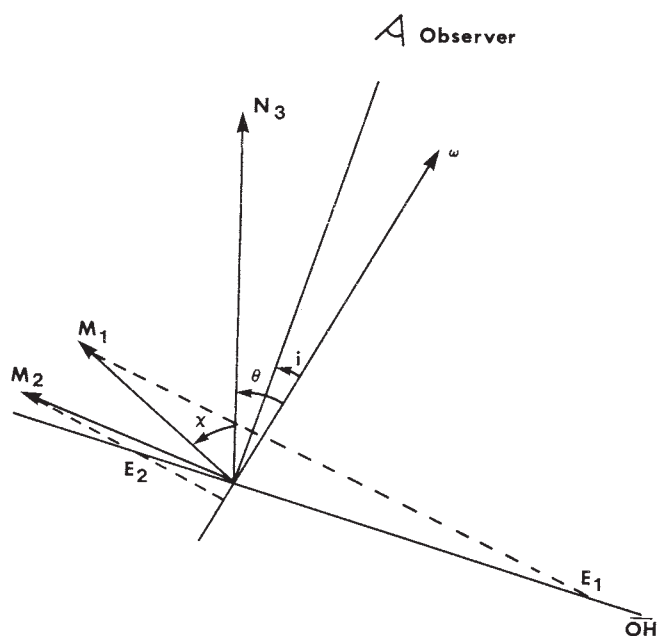


Fig. 1 ω is the total angular velocity vector making an angle i with the line of sight. $\overline{OH}$ is the diameter of the great circle representing the observer's horizon on the neutron star. M_1 and M_2 are the radius vectors to the single hot spot on the surface at two different times and E_1M_1 , E_2M_2 represent the corresponding visible emission bands.

($\sim \delta\alpha/(H_e + \delta) \ll 1$ where $\alpha = \theta_{\max}$, $H_e \equiv H_{1e}$ is the effective dynamical ellipticity and $\delta = H_{2e} - H_{1e}$ is a measure of the triaxiality³ in the case where the 3 axis is taken to be the dominant mass axis), the arc of the emission band that is visible to the observer can change markedly as indicated by the different intersections E_1M_1 , E_2M_2 . It may be shown that the eclipsed arc S is given by $\tan S/2 = \tan(\chi + \theta) \tan i$, and hence that $dS/d\theta = 2 \tan i / (\cos^2(\chi + \theta) + \sin^2(\chi + \theta) \tan^2 i)$. Thus when $\chi + \theta_{\min} \simeq \pi/2$ the fractional change in the observed arc length over a nutation period is, for small angles, $\Delta S/S \sim (\delta/(\dot{H}_e + \delta)) \alpha/i$. For the nutation to be effective in modulating the X-ray emission, this should be roughly equal to the observed depth of modulation in the X-ray flux $\Delta F_x/F_x \sim 0.7$. Thus it is clear that we require $i \sim (\delta/(\dot{H}_e + \delta)) \alpha$ and our line of sight to Cen XR-3 must be closely aligned with its rotation axis. We would not then expect to see a radio pulsar, because the radio beaming is generally thought to originate at a large angle with ω , near the magnetic axis. We also note the possi-

bility of $\theta_{\min} \rightarrow \sim -\chi$, $+\theta_{\min} \simeq 0$. This is the aligned-oblique rotator configuration with ω precessing on the body cone containing m^5 and $\chi \gtrsim 55^\circ$. Here we find $\Delta S/S \sim (\delta/(\dot{H}_e + \delta)) \alpha/(\pi/2 - i)$ so that the line of sight would have to be nearly perpendicular to ω , and we would expect at least the normal chance of observing a radio pulsar. In this case, however, the neutron star crust must be an almost perfect Coulomb crystal in order to support the strain of such a large wobble amplitude without exceeding its elastic limit and flowing plastically to a new equilibrium configuration. Although possible, this model seems rather unlikely, so we shall not consider it further at this time. It is clear that the angular extent of the spot (thickness of the emission band) would have to be $\lesssim (\delta/(\dot{H}_e + \delta)) \alpha$ with a high duty cycle more likely when the equality holds. Of course this mechanism will not work if there are two equally intense hot spots on either end of a diameter (for example, a magnetic axis). The reason is that as one band is eclipsed the other emerges. But although we expect hot spots to form near magnetic poles, there is no reason to suppose that the field is symmetric about a plane passing through the centre of the star. An axially displaced dipole model has been used to explain the observed asymmetric properties of several magnetic stars⁶⁻⁸. Such a model would have important implications for the formation of hot spots. If the displacement of the magnetic dipole along its symmetry axis is by a distance a_R (in units of the stellar radius R), then the ratio of the polar fields is $B_+/B_- = (1 + a_R)^3/(1 - a_R)^3$. Hence, the effective thermal conductivity along the field might be enhanced (over the conductivity normal to the field) by an additional factor of $(1 + a_R)^6/(1 - a_R)^6$ at the $+$ pole relative to the $-$ pole. Moreover, one can show that the angle subtended at the centre of the star by the $+$ polar region (that is, the region where field lines pass outwardly through the stellar surface) is given by $(a_R^2 \neq 0) \sin^2 \phi = (2/a_R^2) [-(1 + a_R^4) + (1 + a_R^2) \sqrt{a_R^4 - a_R^2 + 1}]$. In three magnetic stars, a_R has been found to range from ~ 0.1 to ~ 0.6 ⁶⁻⁸ yielding ϕ in the range $\sim 81^\circ$ to $\sim 36^\circ$. Here we shall require $\phi \lesssim \alpha\delta/(\dot{H}_e + \delta)$ which leads to rather larger values of a_R (for example, $a_R \sim 0.93$ for $\phi \sim 10^{-1}$ radians, $a_R \sim 0.99$ for $\phi \sim 10^{-2}$ radians). This would place the effective sources of the field at, or just outside, the core-mantle boundary in typical neutron stars, which might be the result of flux exclusion from a super-conducting core, or the loss of the outer layers of a main sequence magnetic star in the course of its evolution into a neutron star.

The Nutation Period

The free nutation period can be written in the limit of small angles⁹ as

$$P_N \simeq P/2 \sqrt{H_e(H_e + \delta)} \quad (1)$$

where P is the spin period about the 3 axis. We shall set $\delta \sim H_e$ (see below) and take H_e as the remanent ellipticity⁵ given by $H_e \simeq 1.4 \times 10^{-12} \mu_{30} a_6^2 M^{-3} (2\pi)^2 P^{-2}$ where μ_{30} is the (uniform) rigidity of the crust in units of 10^{30} dynes cm^{-2} , a_6 is the "effective" radius of the crust in units of 10^6 cm and M is the mass of the star in units of the solar mass. If we now identify P_N with the observed X-ray periodicity of ~ 4.87 s, and if we set $a_6 \sim 1.2$, then we find from equation (1) that

$$P \simeq 1.4 \mu_{30}^{1/3} M^{-1} \text{ ms} \quad (2)$$

The expected equivalent black-body temperature can be written as

$$T_s \simeq 3.24 \times 10^9 (4\pi g/\Omega_s)^{1/4} (M \xi^2 P^{-3} \dot{P})^{1/4} \langle \theta^2 \rangle^{1/4} \text{ K} \quad (3)$$

Here, ξ is the radius of gyration in units of the stellar radius; the time average is over a nutation period so that $\langle \theta^2 \rangle \simeq$

$\theta_{\max}^2 (1 - \delta/2(H_e + \delta)) = \alpha^2 (1 - \delta/2(H_e + \delta))$; Ω_s is the solid angle of the hot spot; and g is a gyroscopic factor that is $\approx \frac{1}{2}$ for $\chi \approx \pi/2$. But this expression for T_s has been written assuming that the entire amount of heat generated in the crust would emerge from the spot. This is unlikely to be the case and, in general, we might expect only a fraction $\Omega_c/4\pi$ of the heat generated to be channelled (for example, by the magnetic field) through the hot spot, where Ω_c is then the effective collection solid angle for the spot. One model of the field that achieves this situation is provided by a point dipole situated in the crust. We assume that the core excludes the field lines so that there is an image dipole interior to the core-mantle boundary. The region of magnetic field in the crust enclosed by the same limiting field line as was discussed above is effectively flattened and extended as a result and may be thought of as yielding the collection solid angle Ω_c . Thus the 4π in the equation for T_s should be replaced by Ω_c .

It can be seen also that

$$\Omega_s T_s^4 \lesssim \Omega_c T_b^4 \quad (4)$$

where T_b is the mean temperature of the stellar surface outside the spot ($4\pi - \Omega_c$). This result constrains the background flux to be greater, by a factor $\sim (2\pi - \Omega_c)/\Omega_c$, than the spot flux. We must therefore take care to arrange that T_b is considerably smaller than T_s so that modulation of the soft X-ray flux occurs as the spot visibility changes.

From these arguments for effective modulation, we expect $\Omega_s \approx \pi\alpha^2 (\delta/(H_e + \delta))^2$. The appropriate value to choose for T_s in formula (3) is the equilibrium or quiescent value. Unfortunately it is not clear what this value is, as the most recent measurement¹ is appropriate to post-flare conditions. An earlier determination by the Leicester group¹⁰ gave a black-body temperature of $\sim 2.3 \times 10^7$ K. However, the spectrum at the high-energy end was not determined from simultaneous observations, and from the photon count it would seem that flaring was also present during this observation. Perhaps the best procedure at this juncture is to combine the observed factor 10 increase in photon flux in the 2–6 keV band with the observed flare temperature ($\sim 3.3 \times 10^7$ K) to deduce a pre-flare value. This leads to $T_s \sim 1.5 \times 10^7$ K.

We proceed now by using equation (2) with equation (3) to find

$$\dot{P} \approx 0.5 \times 10^{-18} \mu_{30} M^{-4} \Omega_c^{-1} \xi^{-2} T_{s7}^4 \quad (5)$$

where T_{s7} is the quiescent spot temperature in units of 10^7 K (~ 1.5). From equation (4) the corresponding expression for T_{b7} becomes

$$T_{b7} \approx \alpha^{\frac{1}{2}} \Omega_c^{\frac{1}{2}} T_{s7} \quad (6)$$

It is likely that $\mu_{30} \lesssim 1$, $M \gtrsim 0.1$ and $\xi^2 \gtrsim 0.01$, so we have the limits, $P \lesssim 14$ ms, $\dot{P} \lesssim 5 \times 10^{-13} T_{s7}^4 \Omega_c^{-1} \text{ s s}^{-1}$. We shall attempt to sharpen the estimates below when we discuss the secular behaviour.

Distance to Cen XR-3

We may with this model immediately estimate the distance, D , to Cen XR-3 from $2\pi D^2 F_m \approx 9.0 \times 10^{31} T_{s7}^3 \Omega_s R^2$, where F_m is the observed X-ray energy flux in unit bandwidth at maximum (in keV/keV cm² s) and T_{s7} is the spot temperature in units of 10^7 K. Thus, on using the previous estimate for Ω_s , we find

$$D \approx T_{s7}^{3/2} \alpha R_6 F_m^{-1/4} \text{ kpc} \quad (7)$$

With $T_{s7} \approx 3.3$ and $F_m \approx 1.0$ keV/keV cm² s as observed¹ (post-flare), this becomes

$$D \approx 6 \alpha R_6 \text{ kpc} \quad (8)$$

Our detailed calculations suggest that X-ray flaring and spin changes of both signs are expected if our model applies to objects like Cen XR-3. Fluctuations (as opposed to the regular nutation) in the wobble amplitude can be induced by relaxation of the wobble through plastic distortion and subsequent re-excitation through core-mantle coupling, when the maximum wobble angle α is near the elastic limit $\alpha_L \sim 2\phi_m/H_f$ (ϕ_m is the maximum angle of elastic shear and H_f is the fluid part of the ellipticity). These wobble changes release heat energy ΔW given by $\Delta W \approx 1.4 \times 10^{33} \omega^4 \mu_{30} a_0^3 M^{-2} \Delta(\alpha^2) \text{ erg}$. For Cen XR-3 (equation (2)) this becomes $\Delta W \approx 3 \times 10^{48} M \mu_{30}^{-1/3} \Delta(\alpha^2) \text{ erg}$, which over a thermal diffusion time τ_D produces a mean "flaring" flux

$$\Delta W/\tau_D \approx 3 \times 10^{48} M^2 \mu_{30}^{-1/3} \tau_D^{-1} \Delta(\alpha^2) \quad (9)$$

Taking $\tau_D \sim 10^5$ s (the post-flare flux was observed to be approximately constant for at least 1 day), and taking $\Delta(\alpha^2) \sim 10^{-5}$ ($\lesssim \alpha_L^2$), $\mu_{30} \lesssim 1$, and $M \gtrsim 0.1$, we find $\Delta W/\tau_D \gtrsim 3 \times 10^{36} \text{ erg s}^{-1}$. From equation (8) we find $D \sim 0.6$ kpc, with $\alpha \sim \alpha_L \sim 10^{-2}$ and $R_6 \sim 10$ corresponding to $M \sim 0.1$, for which the flare flux required¹ is $\approx 10^{36} \text{ erg s}^{-1}$. Thus the energy requirement can be met by the wobble changes even in this most stringent minimum mass limit. (This model is refined below.)

Spin Changes

The associated spin changes, however, are for the present case only of order $\Delta P/P \sim 5.3 \times 10^{-9} M^{-2} \mu_{30}^{2/3} \Delta(\alpha^2)$, or $\lesssim 5.3 \times 10^{-7} \Delta(\alpha^2)$. As the observed values are $\sim 2-4 \times 10^{-4}$, this last result clearly does not suffice. But this may be the same circumstance that arises in the Crab¹¹ and Vela objects (reported at URSI meeting, April 1970), where there is a range of spin changes from values of order $\Delta P/P$ given above to the much larger starquakes ($\Delta P/P \sim 10^{-9}$ for Crab, $\sim 10^{-6}$ for Vela). The latter events seem to be quite different phenomena.

The starquake model of the large spinups¹² envisages relaxation in the figure of the star due to "cracking" of the crust. Recently we suggested³ that alternately stressing and relaxing the internal magnetic field can have similar results. This seems capable of explaining both the unusual secular behaviour and the spin changes of Cen XR-3. It follows from equation (1) that

$$\Delta P_N/P_N = \Delta P/P [2 + 1/(1 + \delta/H_e)] - (\Delta\delta/2\delta) \delta/(H_e + \delta) \quad (10)$$

where we have used $H_e \propto P^{-2}$. If $\Delta P/P$ is small, and taking again $\delta \sim H_e$, this becomes approximately $\Delta P_N/P_N \sim -\Delta\delta/4H_e$. If we identify this fractional change in P_N with the observed value of 4×10^{-4} (this occurs over $\sim 10^3$ s, but even if the net change is the sum of numerous smaller ones our arguments apply) then $|\Delta\delta| \lesssim 1.6 \times 10^{-3} H_e$. With $H_e \sim 0.94 \times 10^{-4} M^{-1} \mu_{30}^{1/3}$ for Cen XR-3, this is $|\Delta\delta| \lesssim 1.5 \times 10^{-7} M^{-1} \mu_{30}^{1/2}$. This can be smaller than $\delta_m^{1/3}$ (δ_m is the magnetic contribution to δ ; $\sim 10^{-8}$ for a mean internal field of 10^{13} Gauss and $\sim 10^{-4}$ for a mean internal field of 10^{15} Gauss in a star of $\sim 0.1 M_\odot$) as it must be for the suggestion to be tenable ($|\Delta\delta/\delta| \lesssim \delta_m/\delta$). Thus the magnitude of the spin changes is explicable in terms of fluctuations in the internal magnetic field. We must interpret the spindowns ($\Delta\delta < 0$) in terms of "relaxations" in the stressed field³. The spinups ($\Delta\delta > 0$) are less naturally explained on this model, but possibly they can be understood in terms of irregular yielding (cracking) of the crust. For it is the adjustment of the figure of the star to the new equilibrium figure which we suppose to stress the field and amplify it (see below). If this proceeds irregularly, sudden amplifications can occur. Such sudden yielding is likely to be associated with intervals of plastic deformation and heating, near the elastic limit. We note that in a spindown ($\Delta\delta_m < 0$ and also $\Delta \mathcal{M} < 0$ where $\mathcal{M}$ is the magnetic energy of the star) the magnetic

energy dissipated (presumably in some resistive instability which we cannot hope to specify at this time) is also available for heating. In fact $\mathcal{M} \sim 5/3 \times 10^{43} a_6^3 \bar{B}_{13}^2$ ($\bar{B}_{13}$ is the volume-weighted mean field in units of 10^{13} G), and a comparison with equation (9) shows that such heating could be dominant if a significant fraction of $\mathcal{M}$ is, in fact, dissipated. The spin-down event would then bear a remarkable resemblance to a solar flare.

Secular Behaviour

For secular changes in P_N , equation (10) may be written as

$$\dot{P}_N/P_N = (\dot{P}/P) (2 + 1/(1 + \delta/H_e)) - (\dot{\delta}/\delta)(\delta/(H_e + \delta)) \quad (11)$$

We also write

$$\delta = \delta_m + \delta_f$$

where δ_f is a triaxiality that was established when the crust first formed (presumably on the Jacobi sequence)^{14,15}. We might expect δ_f to be secularly dependent in the same way as H_e ($\propto P^{-2}$) and that $\delta_f \simeq H_e$ (quite possible on the Jacobi sequence). In this way δ can be $\sim H_e$ in accordance with our earlier assumption, even if $\delta_m \ll H_e$.

We shall find below that $\dot{\delta}_m > \dot{H}_e$ which is $\sim \dot{\delta}_f$ so that $\dot{\delta} \sim \dot{\delta}_m$. Moreover, as $\delta_m \sim \mathcal{M}/\Omega$, where Ω is the gravitational energy of the star, we have $\dot{\delta}_m/\delta_m \simeq \dot{\mathcal{M}}/\mathcal{M}$, since the term in $\dot{\Omega}$ is down in magnitude by a factor $\mathcal{M}/\Omega$. As the star adjusts its geometry in the course of the secular slowdown, we expect the field to become stretched and thus amplified at a rate corresponding to the secular release of gravitational energy. Thus we estimate $\dot{\mathcal{M}} \sim -\dot{\Omega}$. But it has also been shown³ that $\dot{\Omega} \simeq (5/8)(\omega^2/\pi G \rho) \dot{E}$ ($\omega^2/\pi G \rho \ll 1$), where ρ is the mean density and $\dot{E}$ is the rate for change of rotational energy. Thus we can write $\dot{\delta}_m/\delta_m \simeq -(5/8)(\omega^2/\pi G \rho)(\dot{E}/\mathcal{M})(\dot{E}/E)$, and hence from equation (11) we have

$$\dot{P}_N/P_N = (\dot{P}/P) [2 + 1/(1 + \delta/H_e)] - (5/4)(\delta_m/(H_e + \delta))(\omega^2/\pi G \rho)(\dot{E}/\mathcal{M}) \quad (12)$$

Clearly either a secular decrease or increase in P_N is predicted, depending on whether or not the second term in the bracket dominates. This term is $\simeq H_e^{-1}(E/(-\Omega))^2$, or $\sim 5 \times 10^2 M^3 \mu_{30}^{-2}$ numerically. This must be large compared with $(2 + 1/(1 + \delta/H_e)) \simeq 2.5$ for the observed secular decrease to be explicable in terms of the nutation period. In view of the uncertainties in our various calculations, we are of the opinion that this number should be at least ~ 10 . Thus we require

$$M \mu_{30}^{-2/3} \gtrsim 0.27 \quad (13)$$

although a smaller limit by up to a factor ~ 1.5 is possible. Thus we can obtain from equation (2)

$$P \lesssim 5.2 \mu_{30}^{-1/3} \text{ ms} \quad (14)$$

We have still, however, to fit $\dot{P}_N/P_N$ to the observed value¹ of $\sim 8 \times 10^{-10} \text{ s}^{-1}$. When condition (13) applies, our predicted value is (from equation (12))

$$\dot{P}_N/P_N \simeq -0.18 \times 10^{-12} \mu_{30}^{-4/3} \Omega_c^{-1} \xi^{-2} T_{s7}^4 \text{ s}^{-1} \quad (15)$$

independent of the mass, although still dependent on the neutron star model through ξ^{-2} . We see that $\Omega_c^{-1} \xi^{-2} T_{s7}^4 \simeq 5 \times 10^3 \mu_{30}^{4/3}$ in order to fit the observed value. Moreover with this result and equations (5) and (13) we deduce

$$\dot{P} \gtrsim 4.7 \times 10^{-13} \mu_{30}^{-1/3} \text{ s s}^{-1} \quad (16)$$

and hence for the age we find

$$P/\dot{P} \gtrsim 1.1 \times 10^{10} \text{ s} \quad (17)$$

Because of the arbitrary factor introduced previously into the mass limit (~ 1.5), ultimate limits are given by multiplying the limits (14), (16) by factors of 1.5 and 5 respectively, and dividing the limit (17) by a factor ~ 3.4 . By taking the upper limit to the mass of stable neutron stars to be $\sim 1.7 M_\odot$, we obtain the other limit in each case as: $P \lesssim 8.2 \mu_{30}^{1/3} \text{ ms}$, $\dot{P} \gtrsim 3 \times 10^{-16} \mu_{30}^{7/3} \text{ s s}^{-1}$ and $P/\dot{P} \lesssim 2.7 \times 10^{12} \mu_{30}^{-2} \text{ s}$. We emphasize, however, that these limits are not very restrictive.

To obtain illustrative values we must consider how the condition $\Omega_c^{-1} \xi^{-2} T_{s7}^4 \simeq 5 \times 10^3 \mu_{30}^{4/3}$ is likely to be met. With $T_{s7} \sim 1.5$ we require $\Omega_c^{-1} \xi^{-2} \simeq 10^3 \mu_{30}^{4/3}$. Moreover, the largest value that it is feasible for Ω_c^{-1} to have is $\lesssim 10$, for then (equation (6) and $\alpha \sim 10^{-2}$) $T_{b7} \lesssim 10^{-0.75} T_{s7}$ and $(2\pi - \Omega_c)/\Omega_c \lesssim 60$, so that the background spectrum does not dominate the spot spectrum in the soft X-ray band. Hence we require $\xi^{-2} \simeq 10^3 \mu_{30}^{4/3} \Omega_c$ with neither μ_{30} nor Ω_c very different from one. Unfortunately this value of ξ^2 suggests neutron star models (unpublished results of G. Baym and D. Pines) in the range 0.1 – $0.15 M_\odot$, which is just where the surface radius (and ξ) begin to vary rapidly. However, typical values are obtained by choosing $M \simeq 0.1 M_\odot$ (and therefore $\mu_{30} \lesssim 10^{-0.65}$ from relation (13) for which $\xi^2 \simeq 1.25 \times 10^2$ ($R_6 \simeq 5.9$)). Therefore we only require (with $\mu_{30} \simeq 10^{-0.65}$) $\Omega_c \sim 10^{-0.23} \simeq 0.59$.

Thus, with equations (14), (16) and (17), we conclude that many of the features of Cen XR-3 can be explained in terms of a neutron star having the following typical properties: $\xi^2 \sim 10^{-2}$, $P \sim 8.5 \text{ ms}$, $\dot{P} \sim 7.8 \times 10^{-13} \text{ s s}^{-1}$, $P/\dot{P} \sim 1.1 \times 10^{10} \text{ s} \sim 370 \text{ yr}$. Moreover, with the particular choice of parameters and model that we have made ($\alpha \sim 10^{-2}$, $R_6 \sim 5.9$) we have the distance to Cen XR-3 as $\sim 350 \text{ pc}$. These numbers are of course to be thought of only as illustrative, but they do suggest, however, that the neutron star which may underlie the X-ray activity of Cen XR-3 is younger than the neutron star associated with the Crab Pulsar NP 0532.

As a final remark we observe that with this theory we can estimate a maximum interval between spin changes as $\sim \mathcal{M}/\dot{\mathcal{M}}$ or $\sim 2(-\dot{\Omega}/\Omega^2)(P/\dot{P})$, which is $\sim 6 \times 10^5 \bar{B}_{13}^2 \text{ s}$ for the model used above. This is typically $\sim 2 \text{ yr}$ ($\bar{B}_{13} \sim 10$), so that there is no difficulty in understanding why these events should have been observed.

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Cell Surveillance Model for Lymphocyte Cooperation

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A model is proposed for clonal proliferation and antibody production of B lymphocytes. It suggests that changes in the B cell surface due to specific antigen binding are detected by T lymphocytes, which then stimulate the B cell to proliferate.

THERE is now extensive evidence that two types of lymphocyte participate in humoral antibody production. In the mouse these cells have been convincingly distinguished by topographical distribution in lymphoid tissues¹, surface markers² and functions (review in ref. 3).

Antibody secretion is limited to cells of the B lymphocyte lineage, which are bone-marrow-derived (Bursa-equivalent) thymus-independent cells. B lymphocytes constitute the progenitor cells for clones of antibody secreting cells. A second population of lymphocytes are thymus-dependent cells (T cells) which are primarily mediators of cellular immunity but which are also required for the production of antibody by B cell clones. This paper deals with the nature of T cell function in cooperating with B cells for antibody secretion.

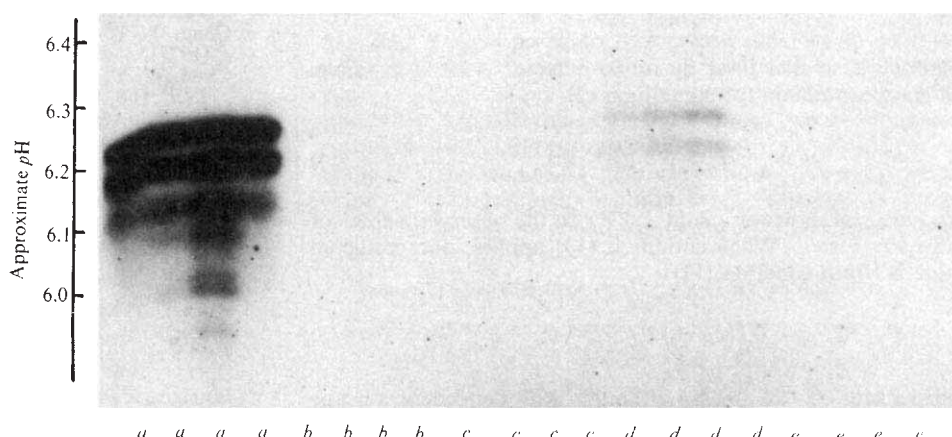
Cooperation between T and B cells has been studied with many antigens, one of the more useful systems being hapten-

protein conjugates. This system can be manipulated so that recognition of the haptenic determinant is predominantly by B cells while T cells recognize chiefly carrier protein determinants⁴. Secondary stimulation of hapten specific B cells is found to be carrier protein specific due to a requirement for carrier primed T cells⁵. From results obtained in this system, it has been proposed that the role of carrier specific T cells is a passive antigen-concentrating device⁶. T cells are envisioned as a means of forming an antigen matrix which is a suitable stimulus for B cell clonal proliferation⁶. There is evidence that this antigen specific, but passive, T cell function is not in itself sufficient⁷.

We have been able to replace carrier-specific T cells by unprimed allogeneic lymphocytes, together with antigen. This allogeneic stimulus is capable of eliciting expression of a single antibody forming clone of B lymphocytes. We interpret our results in terms of a cell-surveillance model for T cell cooperation in B cell function.

The clone (W_1) used in these studies produced a homogeneous IgG₁ kappa anti-DNP. Clone W_1 was isolated by spleen cell transfer in irradiated syngeneic CBA mice after initial priming with DNP-BGG. The clone was maintained by serial spleen cell transfer⁸ and had been maintained through four generations at the beginning of our experiments. The functional marker for clone W_1 was the isoelectric spectrum of anti-DNP in serum (Fig. 1). Propagation of clone W_1 depends on the transfer of memory cells (of B cell lineage) from generation to generation. Proliferation and differentia-

Fig. 1 Isoelectric spectrum of W_1 anti-DNP molecule in serum of irradiated CBA hosts 10 days after transfer of 6×10^6 spleen cells from a W_1 clone bearing CBA mouse together with the following: group *a*, 1 μ g DNP₁₂-BGG; group *b*, 10^6 normal AKR spleen cells—no antigen; group *c*, 10^6 normal CBA spleen cells—no antigen; group *d*, 10^6 normal AKR spleen cells plus 10 μ g DNP₄-OA; group *e*, 10^6 normal CBA spleen cells plus 10 μ g DNP₄-OA. Each letter represents the serum of an individual recipient of the appropriate group. Isoelectric focusing was carried out in 5% polyacrylamide gel with ampholine pH 5-8; the specific pattern of anti-DNP molecules was detected by uptake of radioactive hapten (¹³¹I- α -N-(3,5-diiodo-4-hydroxyphenyl)- ϵ -N-(2,4-dinitrophenyl)-L-lysine and autoradiography. Details can be found elsewhere^{2,3}.



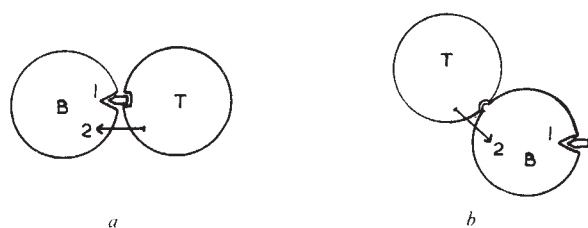


Fig. 2 Models for cell-cell interaction in the cooperation of T lymphocytes in triggering B lymphocytes to produce antibody. Cell-cell interaction effected by an antigen bridge. T and B cells have receptors specific for different determinants on the same antigen molecule. *a*, The antigen concentration-presentation hypothesis postulates multiple antigen molecules bound by T cells and presented to B cells supplying a stimulus (1) sufficient in itself for B cell proliferation and differentiation. Our cell surveillance model postulates binding of antigen by B cell receptors and subsequent recognition by T cells of exposed determinants of the bound antigen; thereupon the T cell supplies an additional stimulus (2) which is probably a soluble mitogen. *b*, Cell-cell interaction effected by histocompatibility antigens. This illustrates the system used in the present experiments to show that allogeneic T lymphocytes can replace antigen specific T cells for cooperation. The B cell requires two distinct stimuli in order to function. Antigen supplies stimulus 1 by binding to specific receptors. Stimulus 2 comes in this case from allogeneic T cells specific for B cell histocompatibility antigens with which they interact.

tion of W_1 memory cells to antibody producing cells are strictly antigen dependent, as described previously for clone E9⁸.

Allogeneic Cells

Clone W_1 shows a pronounced carrier specificity. With the homologous hapten-protein conjugate (DNP₁₂-BGG) only 1 μ g is needed to elicit maximal W_1 antibody production (1–2 mg antibody/ml. serum). By contrast, with 10 μ g DNP₄-OA there is no production of W_1 antibody observed. Ovalbumin was deliberately chosen as a heterologous carrier in the following experiments because it is an exceptionally poor heterologous carrier in a cell cooperation system⁹.

Carrier specificity in the response of W_1 memory cells to DNP-protein conjugates is due to a requirement for carrier specific T lymphocytes (as shown for clone E9¹⁰). Cooperation between the B and T cells has been envisaged as being mediated by an antigen bridge⁴ (Fig. 2*a*). We proposed that if T cells could be brought into contact with B memory cells by a route independent of antigen (Fig. 2*b*) then production of anti-DNP by W_1 memory cells should be carrier independent. A suitable T cell-B cell interaction should be effected by mixing allogeneic lymphocytes. We chose to use spleen cells from unprimed AKR mice admixed with CBA spleen cells containing W_1 memory cells.

When 6×10^6 spleen cells from a W_1 donor CBA mouse and 10^6 normal AKR spleen cells were injected together with 10 μ g DNP-OA into each irradiated CBA recipient there was a sera at 10 days after transfer as shown in Fig. 1. The combinations of cells used in this and subsequent experiments are summarized in Table 1.

This experiment shows that carrier specificity can be overcome by an allogeneic stimulus, suggesting that unprimed allogeneic T cells are substituting for the role played by carrier specific T cells. The allogeneic stimulus does not override the need for a specific DNP stimulus as there is no antibody in the absence of DNP-OA. AKR cells could not be replaced by an equal number (1×10^6) normal CBA spleen cells, or in subsequent experiments by up to 6×10^6 CBA spleen cells. The administration of antigen (DNP-OA) has in other experiments been delayed for up to 5 days after the mixed AKR-CBA/ W_1 cell transfer without significantly altering the extent of W_1 antibody production. Maximal production of W_1 antibody has not yet been achieved using an allogeneic stimulus. Some killing of W_1 memory cells may occur as a result of AKR T cell aggression. However,

when the group of mice receiving CBA/ W_1 and AKR cells without antigen was subsequently challenged with the original antigen (DNP-BGG), production of W_1 antibody was only slightly less than in the control mice which originally received CBA cells in place of AKR cells. Thus neither W_1 memory cells nor carrier primed T cells have been extensively depleted.

Allogeneic Stimulation of B Memory Cells

We then tested whether allogeneic lymphocytes could stimulate B memory cells in the absence of syngeneic carrier-primed T cells. We therefore eliminated syngeneic T cells from CBA/ W_1 spleen cells before transfer with and without AKR spleen cells. CBA T cells were killed by treatment with AKR-anti- θ_{C3H} antiserum together with complement². The surviving population of CBA/ W_1 B cells after transfer did not produce any W_1 anti-DNP in response to DNP-BGG (10 μ g). This result exactly confirms that obtained with clone E9; in that case it was shown that the anti-DNP response could be restored by the addition of carrier primed syngeneic spleen cells. The addition of 10^6 unprimed AKR spleen cells to 4×10^6 CBA/ W_1 spleen cells pretreated with anti- θ_{C3H} and complement partially restores the production of W_1 anti-DNP in response to DNP-BGG (10 μ g). The level of production was very similar to that in the heterologous carrier experiment (Fig. 1). When the carrier specific T cell stimulus is replaced by an allogeneic stimulus, W_1 memory cells respond equally well to DNP on either the original carrier, BGG or on OA. There is, however, an absolute need for specific antigenic stimulus in addition to the allogeneic stimulus.

Nature of the Allogeneic Stimulus

Though CBA and AKR mice are identical at the major histocompatibility locus ($H-2^k$), they probably differ in several minor transplantation antigens. This is clearly shown by grafting skin between members of the two strains. The mean survival time of AKR skin on CBA recipients and vice versa was 14 days and 14.5 days, respectively. Graft versus host reaction was demonstrated by increased mortality after injecting normal AKR spleen cells into sublethally irradiated CBA mice. Death occurred much earlier if AKR spleen cells from animals previously sensitized against CBA transplantation antigens were used. The graft versus host reaction by 10^6 normal AKR cells is mild. This may be a major factor in

Table 1 Cells Used in the Experiments of this Study

Immunogen	CBA/ W_1 cells* No./recipient	Additional cells CBA † AKR ‡ No./recipient	No. of responding recipients per group ‡
1 μ g DNP ₁₂ BGG	6×10^6		4/4
10 μ g DNP ₄ OA	6×10^6		0/4
10 μ g DNP ₄ OA	6×10^6	10^6	0/4
10 μ g DNP ₄ OA	6×10^6	10^6	3/4
None	6×10^6	10^6	0/4 [3/4*]
10 μ g DNP ₁₂ BGG	4×10^6 W_1 -B§		0/4
10 μ g DNP ₁₂ BGG	4×10^6 W_1 -B§	10^6	3/4

* Spleen cells from a W_1 clone bearing mouse.

† Spleen cells from unprimed CBA or normal AKR mice.

‡ Cell mixtures with or without antigen (total volume: 0.2 ml.) injected into the tail vein of previously irradiated (660 r) CBA mice. Recipients bled 10–12 days after transfer. Antibody assayed by the isoelectrofocusing method²³.

§ W_1 spleen cells treated with AKR anti- θ_{C3H} antiserum and complement.

¶ Challenged with 10 μ g DNP₁₂ BGG 17 days after transfer.

Abbreviations: dinitrophenyl-bovine gammaglobulin (DNP₁₂ BGG), dinitrophenyl-ovalbumin (DNP₄ OA). Subscripts refer to mol of hapten/mol of protein.

permitting us to see the allogeneic stimulus of W_1 memory cells without overwhelming cytotoxic effects.

Effector T lymphocytes in graft versus host are capable of producing a range of soluble factors (lymphokines); one product is a mitogenic factor¹⁰. This mitogenic factor is the most reasonable candidate for the T cell stimulus required in addition to antigen for the proliferation and differentiation of B memory cells.

Evidence that helper cells act through a soluble factor comes from studies on *in vitro* antibody synthesizing systems. In the restricted micro-environment of a culture vessel, T cells primed against one species of foreign erythrocyte can "help" an antibody response directed against non-cross-reacting erythrocytes¹¹. Both types of erythrocyte must be present but no antigen bridge can bring T and B cells together. As this type of effect cannot be engineered *in vivo*, the soluble stimulus has apparently a short range of action.

In guinea-pigs primed with DNP-OA, injection of allogeneic lymphoid cells stimulates synthesis of antibodies to both hapten and carrier in the absence of a secondary administration of antigen¹². The anti-DNP response in these animals could be greatly enhanced by a DNP-heterologous-carrier-protein conjugate. These workers showed that the allogeneic effect was a result of graft versus host reactions but could not show an absolute requirement for antigen¹².

Cell Surveillance Hypothesis

On the basis of our studies and others quoted above, we propose a model to explain T cell cooperation in terms of the other known functions of T lymphocytes. Thymus dependent lymphocytes are mediators of cellular immunity. These cells are involved in such reactions as delayed-type hypersensitivity and graft rejection. Burnet has advanced the idea of surveillance as the basic role of T cells¹³. They operate by scanning the antigenic pattern of the body's cells. Cells with altered antigenic structures (such as certain cancer cells) are recognized as foreign and subsequently killed. One of the particular features of this system is that recognition and elimination are brought about by direct or very close cell-cell contact and that circulating humoral antibody is not involved.

The role of T cells as helpers in the B cell antibody response also involves cell-cell interaction⁴, which is usually mediated by antigen (Fig. 2a).

Recent evidence¹⁴ supports the idea¹⁵ that carrier specific T helper cells are identical to delayed-type hypersensitivity effector cells. Our studies clearly show that cooperation can be achieved when T cells contact B cells through histocompatibility specificities (Fig. 2b). We conclude that for clonal proliferation and antibody production B memory cells require two distinct events: (1) a specific antigenic stimulus, (2) a T lymphocyte mediated stimulus, probably of a mitogenic nature.

We envisage the normal sequence of events in a secondary response to a thymus-dependent antigen as follows. Antigens are firmly bound to specific receptors of B cells which are more adequately equipped for this purpose than T cells in the sense that there is probably a higher density of receptors on their membranes^{16,17}. Bound antigens will alter the antigenic composition of the B cell membrane. We assume that under normal conditions T cells are constantly scanning all other cells including B cells. Surveying cells with appropriate receptors will recognize as foreign an immunogen bound to specific surface receptors on B lymphocytes. The T cells then release a factor providing a stimulus to the B cell for clonal proliferation.

In the hapten-carrier system, with low hapten substitution, all haptenic groups can be bound by B cell receptors leaving only carrier determinants to be surveyed by T cells. This explains the requirement for carrier specific helper cells. The surveillance hypothesis can also offer an explanation why hapten-carrier cooperation is unaffected by 100–200-fold

excess of soluble carrier proteins⁹. The antigen concentration hypothesis would predict that the formation of a hapten-protein matrix on protein specific T cells would be very susceptible to inhibition by carrier protein. On the contrary, the surveillance model may require as few as one antigenic bridge between B cell and T cell.

Our hypothesis makes it unnecessary to devise two divergent mechanisms for T cell action: one for killing foreign target cells and a different one for T cell : B cell cooperation. In our system of a single CBA B cell clone with normal AKR spleen cells, a delicate balance between killing and stimulation of B cells has apparently been established. When the number of normal AKR spleen cells was increased or spleen cells were used from animals previously sensitized against CBA transplantation antigens or even primed with ovalbumin and *Bacillus pertussis*, most of the animals died within the first week after transfer. Those recipients surviving for a longer period had no detectable serum antibody. It is reasonable to assume that in this model, killing and stimulation are two manifestations of the same fundamental process which consists of AKR T cells surveying CBA target cells including W_1 B cells.

The cell surveillance model is consistent not only with the data on positive T cell cooperation but also with recent evidence suggesting negative cooperation^{18,19} and other controlling functions mediated by T cells²⁰. Our results can readily explain the reported abrogation of sheep erythrocyte tolerance in rats by administration of allogeneic lymphocytes²¹. Another model system which can be interpreted in terms of our data is chronic allogeneic disease in F_1 hybrids where parental lymphocytes induce host B cells to proliferate and produce auto-antibodies²².

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LETTERS TO NATURE

PHYSICAL SCIENCES

Observation of Volcanic Eruption by an Infrared Radiation Meter

ON September 18, 1970, eruptive activity was initiated from one of the central cones of Akita-komaga-take. The eruption continued for almost 4 months, though mild in activity, and a lava flow extended towards the south-west of the cone.

Akita-komaga-take is located at the northern part of northern Honshu, Japan, and is 1,637 m in altitude. The volcano has a horseshoe-shaped crater at the summit within which three central cones exist. One of the three cones, Medake (1,500 m in altitude), has six explosion pits on its summit. The known records of eruption are 1891, 1902 and 1932, all of which were known as steam eruptions.

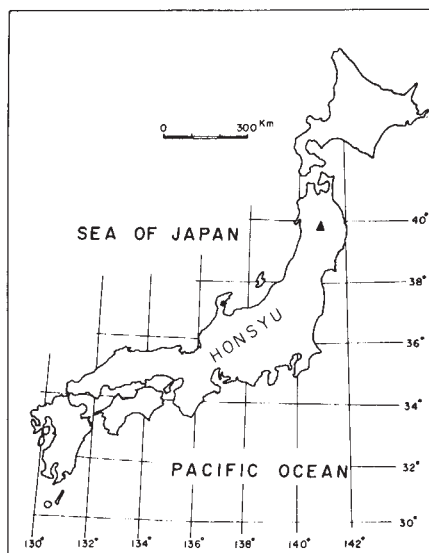


Fig. 1 Location of the volcano Akita-komaga-take indicated.

The recent eruption began from one of the vents of the summit of Medake. The eruptive manifestation is intermediate between Strombolian and Vulcanian type. During the early stage of the activity, the eruption took place at intervals of several minutes. An eyewitness observation has been made occasionally at one of the peaks of the Somma O-dake, 500 m north of the eruptive vent, from which the lava head of the vent was clearly observed without any risk from falling ejecta.

We observed the eruption with an infrared radiation meter in order to determine the accurate time of the eruption and to find a correlation between the pattern of infrared radiation and the earthquakes associated with the particular eruption.

One week after the beginning of the eruption, we set up eight seismometers on the western flank of the volcano, along the line of the steepest slope. Two of them were monitoring seismographs, with smoked paper recording, and the others were temporary, monitoring by a data recorder of magnetic tape or an optical oscillograph with high magnification.

Detailed results of the seismic observation are not described here. But particularly interesting results from the seismic observation are that most of the volcanic earthquakes were those associated with respective eruption. Volcanic earthquakes of different origin have been quite few in number.



Fig. 2 Minor eruption of Akita-komaga-take.

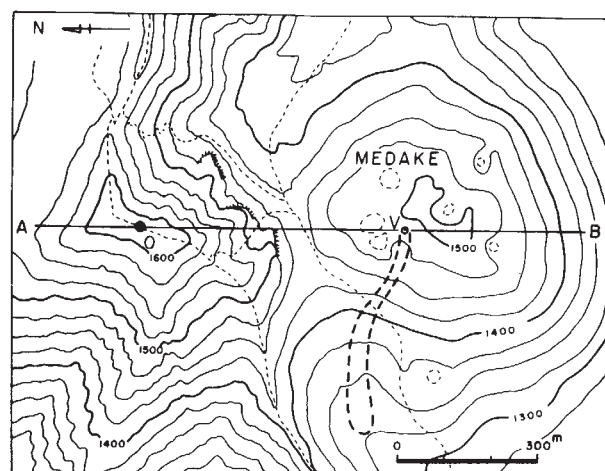


Fig. 3 Topography of Akita-komaga-take and its profile along the line A-B. Location of an infrared radiation meter and the eruptive vent is indicated by O and V, respectively. Dotted line is a rough sketch of the lava flow in its early stages.

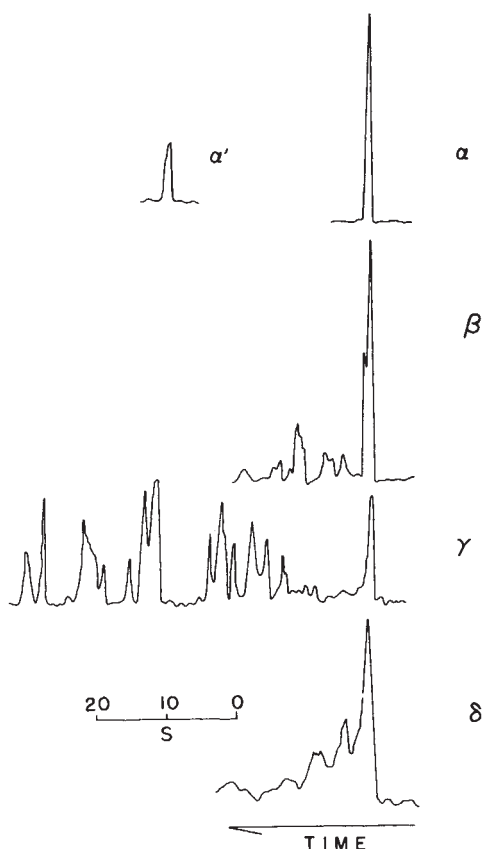


Fig. 4 Types of eruption based on the IRM records.

Besides seismic observation, we set up a portable infrared radiation meter (IRM) on one of the peaks of the Somma O-dake, 500 m from the eruptive vent.

The optical window of the IRM is between $1.8 \mu\text{m}$ and $2.5 \mu\text{m}$. The output signal was recorded by a pen-writing milli-voltmeter. The IRM is designed for industrial use, so the field of view is not narrow enough to observe the surface temperature of the lava head of the vent, or that of the eruptive cloud, at such distances. The observed area was estimated as roughly 6 m in diameter at a distance of 500 m. Therefore the recorded temperature might be the average temperature of the field of view. Time marks of seconds and minutes from the crystal clock were fed onto the recording paper, for which a paper speed of 3 mm/s was selected. A Honda 300 W gasoline generator was used as the power supply.

We obtained many records of eruptions, from which we can classify the eruptive manifestation as follows.

Type α : single eruption ejecting lava fragments and ash with volcanic cloud and strong sound; α' : small single eruption, mostly of gas emission; β : single eruption followed by successive minor eruptions ($\beta = \alpha + \alpha'$); γ : successive minor eruptions ($\gamma = n\alpha'$); δ : ejection of lava fragments with only a small initial velocity. No earthquakes are associated.

Typical records corresponding to the above classification are shown in Fig. 4. The amplitude of the record is proportional to the surface temperature and we can obtain the temperature scale if we assume the emissivity of the source. Because of the relatively wide field of the optical system, we could not determine the actual surface temperature of the lava head of the vent or of the volcanic cloud. In this connexion, the ordinate should be read as the modified temperature scale. It should be said, however, that the integration of the record provides a quantity proportional to the thermal energy released by the particular eruption.

In the early stage of the activity, the IRM record showed a large amplitude and long duration compared with that of the

later stage. An example for both stages is shown in Fig. 5, in which the change in character of eruptive activity with time is well shown. It may be said that observation by IRM provides a precautionary measure of the future volcanic activity.

To investigate the nature of earthquakes associated with volcanic eruption, simultaneous observation of earthquakes and infrared radiation has been carried out. Earthquakes were recorded by a four-channel data recorder with three vertical seismometers located along the steepest slope of the volcano, while the IRM was operated on the summit. Seismographs should have been operated on the summit in order to utilize the crystal clock which was fed to the IRM. But, owing to the difficulties of operating seismographs on the summit, we were forced to make a simultaneous observation with two crystal clocks at different places. In this circumstance, examination of the difference in the time of the first onset of both records involves error, even if we check the two clocks. Certainly, these problems are very important in the

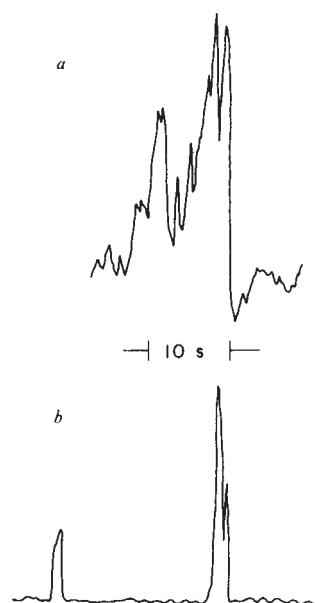


Fig. 5 Eruption recorded by the IRM in the early stage (A) and later stage (B).

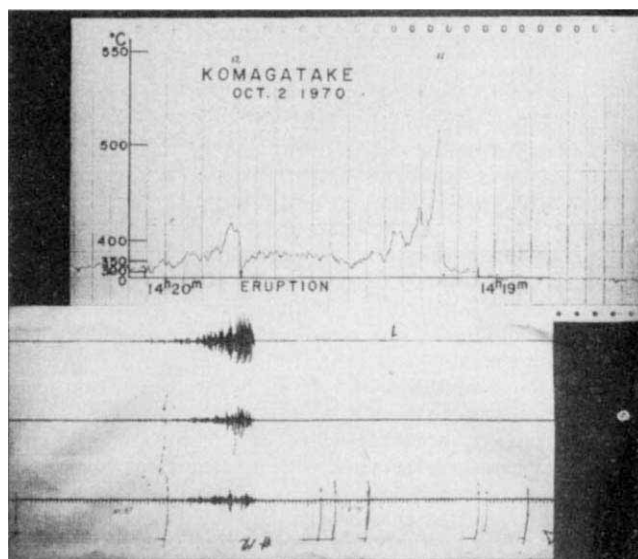


Fig. 6 Upper record of IRM shows two eruptions. An associated explosion earthquake was recorded only for the later eruption.

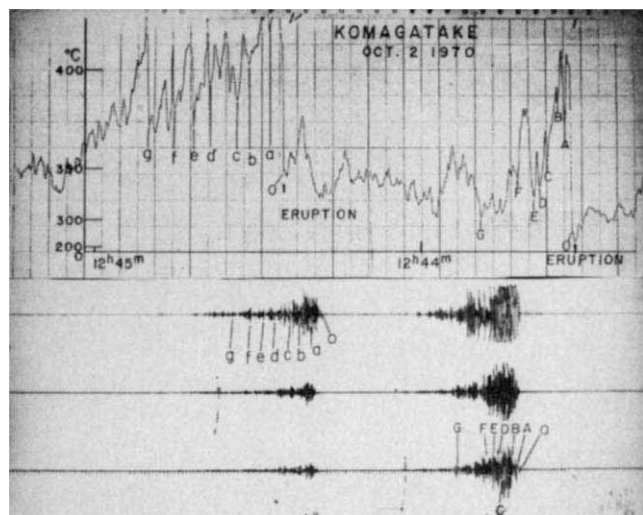


Fig. 7 Correspondence of phases between IRM record and seismogram.

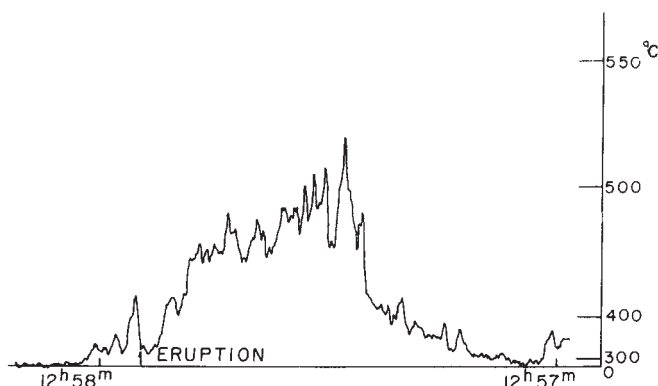


Fig. 8 IRM record of the swelling of the lava head of the vent before the eruption (October 2, 1970).

study of the eruptive mechanism, that is, the depth at which the explosion earthquakes occur. In the case of the present observations, we obtained insufficient results. But, relatively speaking, the time difference is not always the same. It implies a difference in the focal depth of explosion earthquakes for the different eruptions.

The following results are most interesting in regard to the investigation of eruptive mechanisms.

(1) In Fig. 6, two eruptions are recorded by IRM (upper part), eruption No. 11 and No. 12. The explosion earthquakes were only recorded for No. 12 eruption. The time corresponding to the onset of eruption No. 11 is indicated by an arrow on the seismogram. It is clear, from eyewitness observation, that No. 12 was an eruption of the type which ejected lava fragments only. It was found that this type of eruption was not associated with earthquakes.

(2) In Fig. 7, two eruptions are recorded on both an IRM and a seismogram. As can be seen from the IRM record, each eruption is made up of several successive minor eruptions. Major phases are marked by alphabetical letters.

On the other hand, if we make the time of onset of the IRM equal to the time of the first arrival of the seismogram, and transform the time of phases (*A, B, C, . . . , a, b, c, . . .*) of IRM to the seismogram, we can find the distinct phases, as can be seen in the lower part of Fig. 7. From this fact, later phases of explosion earthquakes, so far as we observed, are found to be associated with the respective succeeding eruptions.

Furthermore, in Fig. 7 the thermal energy released by the second eruption is larger than that of the first eruption. On

the other hand, earthquakes associated with these eruptions show the opposite relation—energy released as seismic waves is larger for the first eruption than the second eruption. This implies a considerable difference in the partition of eruptive energy for the two eruptions.

(3) IRM records show another interesting feature. Before a particular eruption, the head of the lava column at the vent shows a vertical oscillation which was clearly observed by the IRM, as shown in Fig. 8. This example shows the upward movement of the lava column in the vent, some 40–50 s before the eruption. The IRM record indicates gradual swelling of the lava column, involving vertical oscillation. This pre-eruptive stage is associated with neither earthquakes nor harmonic tremor.

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Radiography of Diffusion in Gels

IN our study of crystal growth in gels, we have developed a simple technique for monitoring the concentration changes of appropriate ions and molecules in translucent and opaque media by radiography.

The observation of diffusion in aqueous gels by optical methods, for example, the Goüy method¹, is hampered by the absorption, inhomogeneity and frequent opacity of the medium. Indicator methods² require the presence of an indicating reagent which may participate in the diffusion process and which does not necessarily give strict concentration dependence. Radioactive tracer methods have been used for this purpose in the past³ but they entail the use of special reagents and destruction of the medium in order to count specific activity in zones throughout the gel. Other methods involve measurement of the average concentration of diffusant throughout the gel

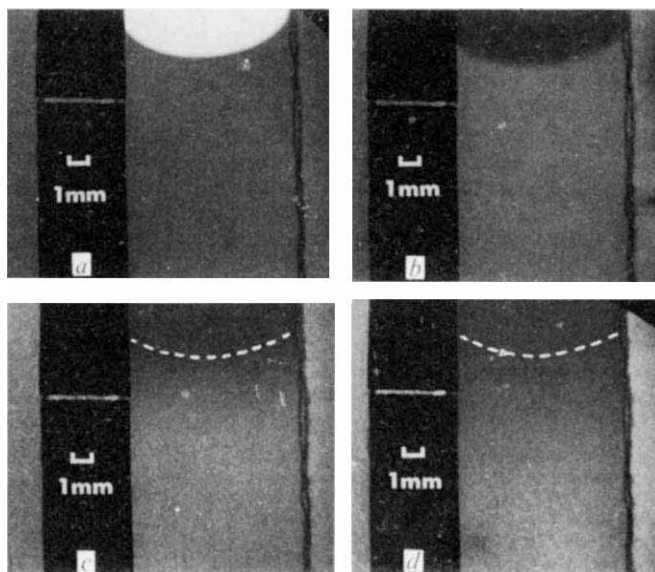


Fig. 1 Radiographs of 1% agar gel. *a*, With air above the boundary; *b*, at $t=0$ min with 0.5 M calcium chloride solution above the boundary; *c*, at $t=38$ min; and *d*, at $t=140$ min. Width of meniscus=8 mm. The exposures of the prints reproduced here have been adjusted to accentuate the contrast in the gel itself in each case.

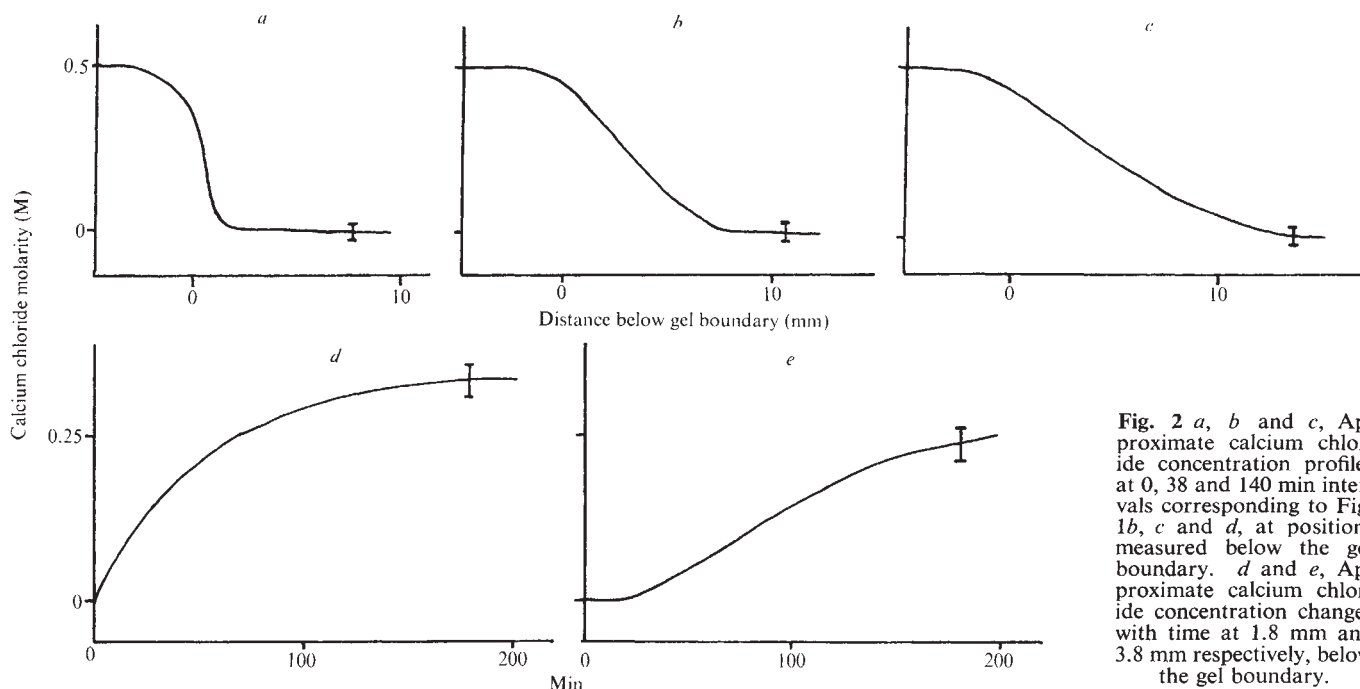


Fig. 2 *a*, *b* and *c*, Approximate calcium chloride concentration profiles at 0, 38 and 140 min intervals corresponding to Fig. 1*b*, *c* and *d*, at positions measured below the gel boundary. *d* and *e*, Approximate calcium chloride concentration changes with time at 1.8 mm and 3.8 mm respectively, below the gel boundary.

or in the diffusing solution^{3,4}; our method provides concentration values at any instant anywhere in a projection of the system.

The diffusion system was contained in a small parallel sided cell 6 × 2.5 cm and 0.32 cm thick, made of 1/16 inch (0.158 cm) 'Perspex' and divided into several compartments. Such a cell is susceptible to variation in thickness due to buckling and bending in preparation, however, and as such is not recommended for quantitative use. A glass cell of similar dimensions can be made to a high degree of accuracy but requires longer exposures owing to the poor X-ray transmission characteristics of most glasses. A conventional crystallographic X-ray tube with a copper target run at 18 kV and 2 mA was used to provide a circle of quasi-monochromated X-rays 6.25 cm² in area, 40 cm from the tube. For the particular cell described exposures of the order of 10 s were required for Kodak fast dental film.

Fig. 1*a* shows the positive of a radiograph of a double cell containing a 1% agar gel. The grey shadow below the meniscus represents the gel with air above it. The dark shadows on the left of the gel are from lead strips which form an intensity and spatial reference. On the extreme left the shadow of the second compartment containing distilled water is visible and, on the right, the shadow of a 'Perspex' spacer at the edge of the cell. The intensities of the latter two shadows, together with that of a 0.0233 mm nickel strip elsewhere on the film, taken relative to the lead shadow, provide a means of standardizing and scaling different films of varying exposure. By using a combination of absorbers of different thickness it is possible to extend this method to obtain scale factors to any desired degree of sophistication.

In Fig. 1*b* a 0.5 M calcium chloride solution has been added above the gel; it appears dark because of the absorption of calcium chloride ions. Figs. 1*c* and *d* show succeeding radiographs at 38 and 140 min intervals from Fig. 1*b* respectively, in which the diffusion of calcium chloride into the gel at 20°C is clearly seen. From a microdensitometer trace of such radiographs it is possible to measure the intensity variation, relative to the internal absorption standards, along the path of the diffusant anywhere on the projection of the gel sample. By obtaining the relative intensities of standard solutions of the diffusant in the cell used from radiographs, the microdensitometer of the diffusion process can be converted to a concentration profile throughout the gel, with a reproducibility of

about 7% down to a concentration of 0.005 M for calcium chloride. Use of a monochromated X-ray source would allow direct calculation of the diffusant concentration from radiograph intensities, using standard atomic absorption coefficients⁵.

Figs. 2*a*, *b* and *c* show concentration profiles corresponding to Figs. 1*b*, *c* and *d*, taken perpendicular to the gel meniscus, at its centre. From a series of concentration profiles, the concentration changes at a particular position in the gel and the movement of a particular concentration contour with respect to time can be obtained. The concentration changes at two positions in the agar gel are shown in Figs. 2*d* and *e*. By using the well known Fourier series solution to Fick's second law of diffusion¹ it is possible to extract a diffusion coefficient from the observed concentration values for the diffusant in the gel medium. No diffusion coefficients are quoted, as preliminary results obtained using a 'Perspex' cell were inconsistent and, as described, we feel more dependable information can be obtained using glass cells.

The method outlined is simple and has been used to obtain preliminary comparative information on the diffusion of ions in a wide variety of gels. We intend to use this method to monitor the growth and dissolution of crystals in gels, especially in connexion with mineralization and demineralization in biological systems, and also in the presence of electric and magnetic fields and as a function of temperature. We hope that it may also be possible to make similar studies of the behaviour of organic crystals in appropriate systems by methods of this kind and suggest that the method may be applicable to a wide variety of diffusion systems, particularly in solution and hot gas flow streams.

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Internal Friction in Moon and Earth Rocks

RECENT measurements of the quality factor, Q , for mechanical vibrations generated by dropping parts of the Apollo lunar module and by moonquakes¹ indicate that the Q of the Moon for these vibrations is of the order of 3,000, but seismological measurements of the Earth's rocks indicate Q s of 110 for the outer mantle², with higher values for the inner mantle. It is the purpose of this report to indicate that these lunar results are consistent with a dislocation source for internal friction.

A dislocation mechanism has been discussed³ which gives an internal friction independent of the frequency for low frequencies and a Granato-Lücke type performance at higher frequencies. The complete normalized internal friction curve is shown in Fig. 1. The normalized frequency ratio (ω/ω_0) is determined by the angular measuring frequency ω divided by $\omega_0 = \mu b^2 / B l_A^2$ where μ is the shear stiffness, b the Burger's distance, B the drag coefficient which measures the force proportional to the velocity exerted on dislocations by phonons and electrons, and l_A is the average distance between impurity pinning points for an exponential distribution. The normalized internal friction ratio determines the ratio of Q^{-1} to $\bar{N} R l_A^2$ where $\bar{N}$ is the total length of dislocation per cm^3 , and R is an orientation factor which determines the ratio of the shearing stress in the glide plane to the stress in the mode of vibration. For a Young's modulus type of vibration, a reasonable value is $R = 0.25$. Assuming, as will be shown likely, that Moon rocks are similar to impure metals and alloys, which have $l_A \div 3 \times 10^{-5}$ cm, gives a Q of 3,000 when $\bar{N} = 5 \times 10^7$, a value consistent with alloys and impure metals.

Two measurements⁴ have been made on Westerley granite and Pennsylvania slate which agree with the form of the theoretical curve of Fig. 1. A third rock Solenhofen limestone has recently been completed with the results shown in Fig. 2. The lower frequency part of this curve and the squares of the high frequency measurements were made by a continuous wave measurement with two equal transducers cemented to a half wave specimen of rock. The resonant frequency determines the elastic modulus while the width of the resonant curve determines the internal friction Q^{-1} . The crosses show measurements by a pulsing method which also gives the velocity as 5.65×10^5 cm s⁻¹. The triangles and circles show other measurements of the same limestone⁵ (triangles) and a different limestone⁶. The agreement is good up to 5 MHz. Above this frequency, scattering losses become large enough to make measurements questionable. As can be seen the dislocation loop length is in the order of 1.9×10^{-4} cm while the number of dislocations is about 10^7 cm⁻². As will be shown here, these values are associated with the numbers expected for free surfaces where l_A is effectively larger and $\bar{N}$ smaller than for a continuous medium.

For a rock most of the internal friction is connected with the grain boundaries as can be seen from the fact that hydrostatic pressure reduces the internal friction by a factor of 10

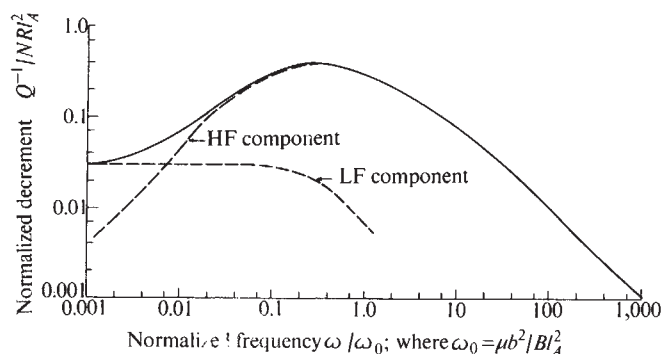


Fig. 1 Normalized internal friction curve against normalized frequency for an exponential distribution of pinning points.

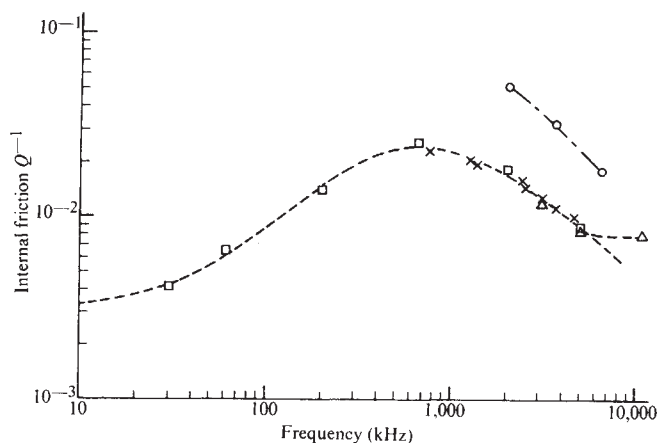


Fig. 2 Internal friction measurements for Solenhofen limestone. $\circ$, Krishnamurthi and Balakrishna; $\triangle$, Peselnick and Zeitz; $\square$, continuous wave measurement; $\times$, pulsing measurement; ---, theoretical.

or more^{7,8}. A model is used here for which a grain boundary is represented by a series of spherical surfaces having a shearing modulus $\mu(1 + j/Q_1)$ pressing on each other, while the interior is represented by a solid having a stiffness $\mu[1 + j/Q_2]$. Making use of the Hertz theory of contacts and Mindlin's⁹ calculations of the displacement of a set of convex surfaces, then for an applied alternating force T we find, for the sidewise displacement dx_2 ,

$$dx_2 = \frac{(2 - \sigma)T}{8a\mu(1 + j/Q_1)} \quad (1)$$

The tangential force T is related to the applied shearing stress T_6 by

$$T_6 = T(\pi a^2)n_0 \quad (2)$$

where a is the Hertz radius of contact, and n_0 the total number of contacts per cm^2 . The Hertz radius of contact is determined by

$$a = \sqrt[3]{\frac{3}{4} N r \left(\frac{1 - \sigma}{\mu} \right)} \quad (3)$$

where r is the radius of the spherical contact surfaces and N the normal force on each contact. The product Nr can be eliminated by considering the thickness t_1 of the layer $r\theta^2$ and the area $\pi r^2\theta^2$ where θ is half the angle determined by the radius of curvature r . Then

$$Nr = T_1/n_0\pi t_1 \quad (4)$$

where T_1 is the sum of the pressure holding the rock together, T_1 , plus the applied hydrostatic pressure. Introducing all these values the sidewise displacement is

$$dx_2 = \frac{(2 - \sigma)}{6(1 - \sigma)} \frac{T_6 t_1}{T_1(1 + j/Q_1)} \quad (5)$$

To the displacement of the intermediate layer we have to add that in the two outside layers which result in

$$dx_1 = \frac{T_6 t_2}{\mu(1 + j/Q_2)} \quad (6)$$

Making use of the fact that Q_1^{-1} and Q_2^{-1} are small, the ratio between the applied shearing stress T_6 and the shearing strain

$$S_6 = \frac{2(dx_1 + dx_2)}{t_1 + t_2}$$

is

$$\mu_T = \mu[1 + t_1/t_2]/(1 + A); Q_T^{-1} = Q_2^{-1} - \frac{A(Q_2^{-1} - Q_1^{-1})}{1 + A} \quad (7)$$

where

$$A = \frac{(2 - \sigma)}{6(1 - \sigma)} \frac{\mu}{T_1} \frac{t_1}{t_2}$$

The fact that lunar rocks studied on Earth¹⁰ have a Q of only 10 to 100 is not in contradiction with the high Q measured on the Moon. As shown by equation (7) the Q measured for rocks in the laboratory is mostly determined by grain boundaries and cracks between various parts. It is not until the pressure is high enough to eliminate these that the Q is determined by the internal structure of the rock. Hence the measurements of Warren *et al.*¹⁰ in no way contradict the ideas expressed here.

Using measurements¹¹ of the shear stiffness of Westerley granite (Fig. 3) it is found that $T_{10} = 3 \times 10^8$ dyne cm⁻²; $t_1/t_2 = 1.7 \times 10^{-3}$ and $\mu = 3.57 \times 10^{11}$ dyne cm⁻². Hence for no external pressure the internal friction is

$$Q_T^{-1} = 0.545 Q_2^{-1} + 0.455 Q_1^{-1} \quad (8)$$

while for high pressures it is equal to Q_2^{-1} , the value for the medium. Using the above values and Q_1^{-1} equal to 1.1×10^{-2} to agree with the torsional wave measurements of Birch and Bancroft⁷ on Rockport granite (550×10^{-5} at 200 bar and 60×10^{-5} at 4 kbar) the value due to the boundary is less than 2×10^{-4} at 10 kbar while Q_2^{-1} , the internal friction of rocks without flaws, is less than 2×10^{-4} . Hence granite under pressure has internal friction values of the order of that found for Moon rocks. Because all the seismological measurements and the Moon rock measurements are for hydrostatic pressures large enough to eliminate the effect of the boundary, we are dealing with the internal friction of the medium itself. The question then is why the internal friction of the Moon rocks is so much lower.

The answer seems to be that the shearing stress associated with the ellipticity of the Moon (about 10^7 dyne cm⁻², ref. 12) is insufficient to generate new dislocations by the Frank-Read mechanism whereas the stress in the Earth's crust (from 6×10^7 to 10^8 dyne cm⁻²) is large enough to generate new dislocations. A Frank-Read mechanism starts to function when the applied shearing stress—perhaps tectonic in origin—is

$$T_6 = \frac{\mu b}{l_N} \quad (9)$$

where l_N is the network length, that is, the distance between dislocation joins. The Frank-Read source will operate until

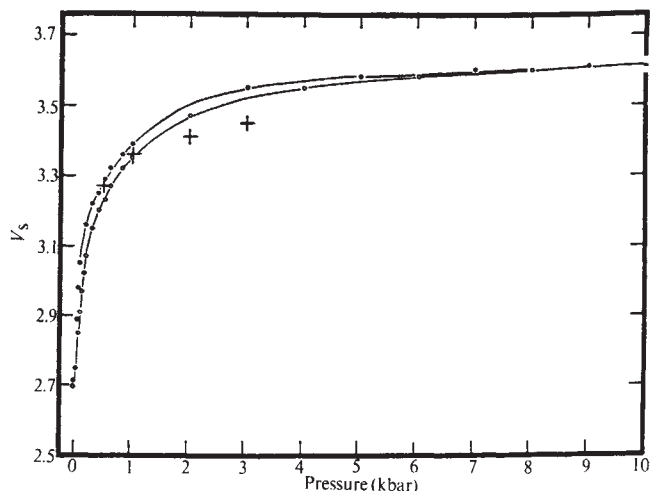


Fig. 3 Shear velocity of Westerley granite as a function of hydrostatic pressure. The lower curve was obtained with increasing pressure. Crosses represent data from Birch and Bancroft (from ref. 11).

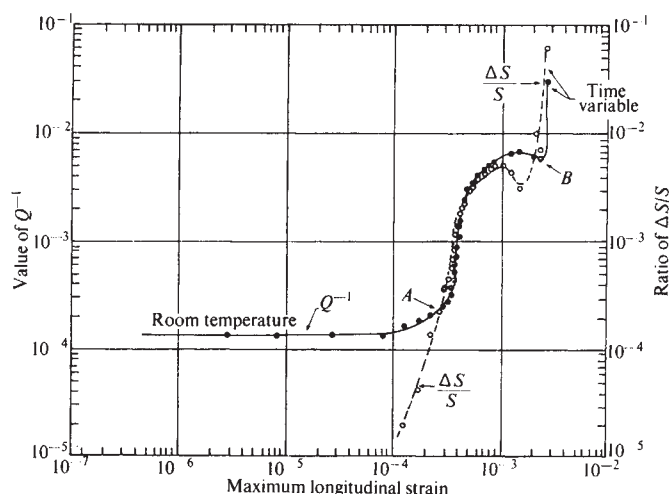


Fig. 4 Internal friction plotted as a function of the maximum longitudinal strain for brass. Slip bands begin to form at a strain of 3×10^{-4} .

the length l_N is equal to $\mu b/T_6$. If some of these loops anneal out, the mill is started again until l_N is determined by $\mu b/T_6$. Some idea of l_N can be obtained for brass from the data of Fig. 4, which shows the internal friction as a function of the longitudinal strain. At about 3×10^{-4} , the dislocation mill starts to move as can be seen by the presence of slip bands¹³. This requires a longitudinal stress of 3×10^8 dyne cm⁻² or, with an orientation factor of 0.25, a shearing stress of 7.5×10^7 dyne cm⁻² which from (9) results in a network length of 1.25×10^{-4} cm. For the rock it might be somewhat larger but would not be large enough to cause the mill to operate at a stress of 10^7 dyne cm⁻². An even stronger argument¹² is that the ellipticity of the Moon has not changed during geological times even though it keeps the same face to the Earth.

For the Earth, the stresses are larger and they occur every 12 h. Thus new dislocations can be generated which will probably be pinned eventually by impurities giving the same pinning distance l_A as in the unstrained rock. The number of dislocations will continue to increase until the joins of the network reduce the network length l_N to a length such that the Frank-Read mechanism can no longer operate. A rough idea of the relation between $\bar{N}$ and l_N can be obtained from the equation

$$\bar{N} l_N^2 = 3$$

as can be seen from a figure showing the network as blocks with dislocations going straight through. For $\bar{N} = 5 \times 10^7$; l_N is 2.3×10^{-4} cm which is not long enough to start a dislocation mill on the Moon. For a Q of 110, the number of dislocations has increased by a factor 27.2 which results in a value of l_N of 4.7×10^{-5} cm. This gives a shearing stress of 1.9×10^8 dyne cm⁻² to cut the mill off. Although all these figures are approximate, they are close to what is expected.

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Time of Formation of the Earth's Core

ACCORDING to several theories¹⁻³, the Earth was formed by accretion from a mixture of silicate particles and metal particles, something like those in chondritic meteorites. The accreting material may also have contained relatively small quantities of other components such as iron meteorites⁴ and carbonaceous chondrites⁵. Most advocates of these theories have maintained that accretion occurred relatively slowly (over a period of about 10^8 yr), so that the gravitational potential energy was efficiently radiated away and the Earth formed in a relatively cool and unmelted condition. Subsequently, heating by long lived radioactive elements occurred, leading to melting of the metal phase and segregation of the core. Differentiation and formation of the core may thus have occurred much later than the primary accretion of the Earth. It has even been argued that core formation might not yet have proceeded to completion^{1,6,7}. On the other hand, alternative theories of origin of the Earth^{8-10,11} have maintained that much of the accretion of the Earth occurred in high temperature conditions and that core formation occurred either during accretion or very soon afterward. Clearly, an independent determination of the time interval ΔT_c between accretion and core formation would have a crucial bearing on these contrasting theories.

The age of the Earth, T_0 , is believed to be close to 4.55×10^9 yr (refs. 12 and 13). This age is based on the discovery that the isotopic composition of modern terrestrial lead falls on the meteoritic isochron, which records a differentiation of lead relative to uranium within meteorites at this time. The mean growth curve for lead ore bodies indicates an age of 4.58×10^9 or 4.55×10^9 yr for the Earth depending on the choice of ores included in the calculation^{14,15}. Both the meteorite and ore body ages assume that the lead/uranium ratio of the upper mantle-crust system had been established at T_0 , and since then had remained constant to a first approximation.

In 1960, Ringwood⁸ argued that formation of the core would probably alter the Pb/U ratio of the mantle because the molten iron would be expected to carry down substantial amounts of lead but not uranium. He concluded, therefore, that the 4.55×10^9 yr terrestrial event recorded by Pb/U geochronology actually referred to the date of core formation, which must have occurred very soon after formation of the Earth.

The principal assumption behind this conclusion was that during core formation the Pb/U ratio of the metal phase would be much higher than that of the silicate phase and that a substantial proportion of the total lead in the Earth would enter the core. This assumption was not accepted by Patterson and Tatsumoto¹⁶, however, who maintained that lead would be preferentially partitioned into the silicate phase. Their calculations of mantle evolution implicitly require that either trivial amounts of lead are partitioned into the core, or that core formation occurred instantaneously 4.55×10^9 yr ago. It is not obvious that either of these implicit assumptions is necessarily true. Clearly, experimental measurements of the distribution coefficient of lead between relevant metal and silicate systems are necessary. We have carried out a series of experiments of this nature. It was difficult to perform these experiments at atmospheric pressure because lead tends to

volatilize at temperatures at which the metallic iron-rich phase is molten. Accordingly, the experiments were performed at high pressures under closed system conditions, thereby preventing volatilization.

The experiments were designed as analogues of the core formation process in the Earth, as envisaged in various theories. The Earth's core is believed to contain 10-30% of one or more light elements (such as S, Si, C) in addition to iron¹⁷⁻¹⁹. According to a widely discussed current theory the Earth accreted mainly from material resembling ordinary chondrites^{3,20-22} together with a small proportion of carbonaceous chondrite like material, which was the "carrier" of volatile components⁵. On this hypothesis the principal light-element component of the core would consist of sulphur with a smaller amount of carbon. We represented this model with a synthetic metal phase of composition (weight per cent) $\text{Fe}_{83}\text{Si}_{15}\text{C}_2$. Another model^{8,19} of core formation regards silicon as the most important light element. To test this model, a metal phase of composition $\text{Fe}_{89}\text{Si}_{11}$ was used. Finally, according to a third hypothesis²³ the Earth's core contains the primordial abundance of sulphur. In the relevant experiment, a metal phase of composition $\text{Fe}_{70}\text{S}_{30}$ was used.

The silicate phase used in the experiments consisted of simplified synthetic basaltic compositions, doped with 1,000 p.p.m. of lead (as oxide) in some runs and 100 p.p.m. Pb in others. For experiments on the $\text{Fe}_{89}\text{Si}_{11}$ -basalt system, the simplified basalt composition contained no iron, whereas the other basaltic compositions used contained normal amounts of oxidized iron.

In the experiments, finely powdered samples of metal phase were mixed with silicate in the ratio 1 part (by weight) of metal to 2 parts silicate. The mixture (400 mg) was then charged into a closed aluminum capsule which was placed in an internally heated piston cylinder high pressure apparatus²⁴. In a given run, the sample was first subjected to the desired pressure. Temperature was then increased to 1,100° C and held for 30 min, to recrystallize and equilibrate the charge under sub-solidus conditions. The temperature was then slowly increased over a period of 1.5 h to a maximum temperature at which both metal and silicate phase were known to be molten. The temperature was held steady for 10 min, and the system was then switched off. Pressure was released after cooling and the samples removed from the apparatus. The metal was usually found to have sunk to the bottom of the capsule as one or more globules. Metal and silicate phases were crushed and separated magnetically. The concentrations of lead in coexisting metal and silicate phases were then determined by isotope dilution with Pb runs as PbI_2^+ (ref. 25). Blanks were approximately 20 ng per analysis.

Distribution coefficients for lead between metal and silicate phases (K^m 's) were obtained for each experiment. The distribution coefficient is the ratio of Pb concentration in the metal to Pb concentration in the silicate phase. Results are shown in Table 1. They are sufficiently consistent to support the belief that equilibrium was approached. The fact that all lead was initially in the silicate phase means that our distribution coefficients represent minimum values.

A series of runs was made at 10 kbar. These illustrate the relation between distribution coefficients and metal phase

Table 1 Distribution Coefficients for Lead between Metal and Silicate Phases

Metal phase	Pressure (kbar)	Maximum temperature (°C)	K^m metal/silicate
$\text{Fe}_{70}\text{S}_{30}$	10	1,400	9.8, 12.4
$\text{Fe}_{89}\text{Si}_{11}$	10	1,400	3.7, 3.8
$\text{Fe}_{83}\text{Si}_{15}\text{C}_2$	10	1,400	1.9, 2.0, 2.1
$\text{Fe}_{83}\text{Si}_{15}\text{C}_2$	5-40	1,350-1,650	1.4 to 2.9
	12 runs over 5 kbar intervals including repeats		Most probable value 2.5 ± 0.5

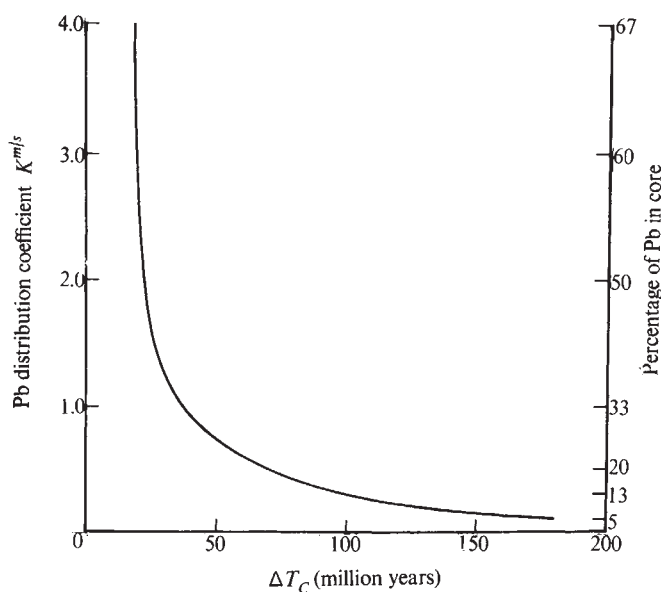


Fig. 1 Relationship of time delay for core formation after accretion of Earth, ΔT_c , with the distribution coefficient for lead between metal and silicate phase, $K^{m/s}$. Time of accretion is taken as 4.55×10^9 yr. Isotopic composition of upper mantle lead is taken as that from tholeiites dredged from floor of Pacific Ocean²⁸.

compositions (Table 1). It is seen that the coefficients for $\text{Fe}_{70}\text{S}_{30}$ and $\text{Fe}_{89}\text{Si}_{11}$ are higher than for the $\text{Fe}_{83}\text{Si}_{15}\text{C}_2$ alloy. Evidently carbon has the effect of increasing the activity of lead in the alloy. A further series of runs was made over a pressure range from 5–40 kbar. The temperature at which equilibration was reached in these runs is not known precisely because the metal (which melts at a lower temperature than the silicate) tends to segregate and sink to the bottom of the capsule when the basalt is melting. Thus the P, T conditions under which equilibration occurred probably correspond approximately to the liquidus of basalt or to the solidus of mantle peridotite²⁶. Conditions of segregation of metal phase are thus analogous to those which may have occurred during core segregation in the Earth. Three runs were carried out at 15 kbar and at maximum temperatures of $1,450^\circ\text{C}$, $1,500^\circ\text{C}$, $1,600^\circ\text{C}$. The scatter was large and no systematic effect of temperature on distribution coefficient was seen. Also, no systematic effect of pressure on the distribution coefficient in the range 5–40 kbar was evident in the runs using the $\text{Fe}_{83}\text{Si}_{15}\text{C}_2$ alloy. There is some scatter in the results, probably due to failure to attain complete equilibrium which would reduce the distribution coefficient values. It is probable, therefore, that the higher values obtained are more nearly correct and our estimate for the distribution coefficient of lead in this system over the ranges $1,350$ – $1,650^\circ\text{C}$ and 5–40 kbar is $K^{m/s} = 2.5 \pm 0.5$.

A relationship exists between the mean metal–silicate distribution coefficient for lead $K^{m/s}$ and the time interval ΔT_c between accretion of the Earth and segregation of the core. To calculate ΔT_c precisely, we need to know, in addition to $K^{m/s}$, (i) T_0 , the age of accretion for the Earth; (ii) the isotopic composition of lead incorporated into the Earth at accretion; (iii) the isotopic composition of lead which has evolved in rock systems which have experienced no change in Pb/U (except as a result of radioactive decay) between the time of core formation and the time of emplacement in their present setting.

A detailed discussion of the relationship between $K^{m/s}$ and ΔT_c and of the boundary conditions (i), (ii) and (iii) is in preparation. For present purposes, a summary will suffice. For the age of accretion of the Earth, we take $T_0 = 4.55 \times 10^9$ yr. For (ii), we take the isotopic composition of lead from

the troilite phase of iron meteorites²⁷ ($\text{Pb}^{206}/\text{Pb}^{204}$) $T_0 = 9.346$ and ($\text{Pb}^{207}/\text{Pb}^{204}$) $T_0 = 10.218$. Finally, for (iii) we chose the isotopic composition of lead from young oceanic tholeiites dredged from the floor of the Pacific Ocean²⁸. Using these boundary conditions the relation between $K^{m/s}$ and ΔT_c is shown in Fig. 1.

The model with a metal phase composition $\text{Fe}_{83}\text{Si}_{15}\text{C}_2$ approximates to a widely discussed class of hypotheses concerning the composition and origin of the Earth (accretion from mixed meteoritic material). The distribution coefficient for this composition is 2.5 and Fig. 1 shows that core segregation must have occurred within about 20 m.y. after accretion. For the $\text{Fe}_{70}\text{S}_{30}$ and $\text{Fe}_{89}\text{Si}_{11}$ compositions, $K^{m/s}$ values are larger; however, because the ΔT_c curve is very steep above $K = 1.5$, the ΔT_c intervals are only slightly shorter. We conclude that the Earth's core formed either during accretion of the Earth or very soon after. Variations in the boundary condition parameters (i), (ii) and (iii) within reasonable and acceptable limits do not affect this basic conclusion. The upper limit to ΔT_c for reasonable sets of boundary conditions is about 2×10^8 yr for $K^{m/s} = 2.5$, as measured.

The only significant uncertainty in these results is the possibility of a substantial negative pressure coefficient of $K^{m/s}$. Our results for the pressure range 0–40 kbar show that a large pressure coefficient does not exist. The scatter in the results could, however, conceal a relatively small positive or negative pressure coefficient. A pressure of 40 kbar is roughly one-tenth of the median pressure in the mantle so that the experimental pressure range over which distribution coefficients were measured is not insignificant. Moreover, plausible theories of core formation^{8,22} involve equilibration of metal and silicate at relatively low pressures in the upper mantle, followed by segregation of metal into large drops which sink to the centre rather rapidly without altering the equilibrium which was established at low pressure. Nevertheless, we plan to extend these measurements to 100 kbar. Quite a large negative coefficient would be necessary to reduce $K^{m/s}$ to 0.4 at 400 kbar. As can be seen from Fig. 1, even with this value core formation must occur in less than 100 m.y. For FeSi and FeS cores, the corresponding time intervals would be much shorter.

We conclude that the time interval ΔT_c between accretion of the Earth and formation of the core was relatively short. Most probably, ΔT_c was smaller than 10^8 yr and almost certainly it was smaller than 5×10^8 yr. Core formation could well have occurred simultaneously with accretion. This constitutes a fundamental boundary condition for theories of the Earth's origin, and strongly favours models in which much of the accretion of the Earth occurred under sustained high temperature conditions^{8–11}. Conversely, models of the type which have prevailed for two decades^{1–7,20–22} and which regard the Earth as having accreted in a cool, unmelted state, followed by subsequent internal heating supported by long lived radioactivities, ultimately culminating in core formation at a much later stage, no longer appear tenable.

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periods are listed in Table 1. With the exception of ${}_2S_2$ all these modes were either misidentified or remained unidentified in Table 5 of Derr⁵. Recently Dratler *et al.*⁶ gave the correct identifications of ${}_2S_0$ and ${}_4S_0$.

Dziewonski⁷ inferred from observations of radial overtones ${}_1S_0$, ${}_2S_0$ and ${}_3S_0$ that introduction of a solid inner core represents the simplest explanation of the observed periods of these modes. The additional evidence gathered since that publication led to a revised model UTD124B' with a value of 3.517 km s^{-1} for the average shear velocity in the inner core⁴. The theoretical periods computed for this model are shown in Table 1. The r.m.s. error of the residuals for all nine modes is 0.12%.

Dziewonski and Gilbert⁴ calculated the energy balance for each mode following the method of Backus and Gilbert⁸. Table 1 shows the compressional and shear energies in the inner core presented as fractions of the total potential energy. The modes in Table 1 can be divided into two groups according to the predominant type of energy. Group 1 consists of modes with mostly compressional energy (${}_1S_0$, ${}_2S_0$, ${}_3S_0$, ${}_4S_0$, ${}_6S_1$, ${}_8S_1$), and the modes with predominantly shear energy belong to group 2 (${}_2S_2$, ${}_5S_2$, ${}_7S_3$); the energy of the mode ${}_8S_1$ is distributed rather evenly.

Although the rigidity of the inner core introduces relatively large non-linear effects, equation (49) of Backus and Gilbert⁸ should predict the correct sign of the change in frequency due to variations of elastic parameters for modes with strongly polarized energy. A decrease in rigidity will increase the bulk modulus and consequently increase the frequency of the modes of group 1, and at the same time decrease the frequency of the modes of group 2 with predominantly shear energy.

To test the correctness of this estimate we have introduced a liquid inner core in the model UTD124B', retaining all the other parameters unaltered. The results of calculations shown in Table 1 confirm in all cases the predicted change in the eigenfrequency. Of all the other modes identified by Dziewonski and Gilbert⁴ only ${}_0S_2$ and ${}_2S_3$ show a 0.2% change due to the modification in the model, and for all remaining modes the change is less than 0.1%. Observations of these modes can therefore be considered irrelevant to the question of the solid against liquid inner core.

The r.m.s. error for the nine modes is practically the same for the three liquid inner core models: modified UTD124B', 5.08M⁹ and HB₁¹⁰ (all of which satisfy the travel time data rather well), and it is more than eight times greater than for model UTD12B'. Also, the sign of the residuals for the individual modes is the same in all cases. This implies that the agreement between the observed data and eigenfrequencies computed for models 5.08M and HB₁ could be significantly improved by the introduction of rigidity in the inner core of these models.

Our argument has shown so far that there is no doubt that solidity of the inner core represents a simple and logical "data consistent" answer. The important question is whether it is a

Solidity of the Inner Core of the Earth inferred from Normal Mode Observations

SOLIDITY of the inner core was originally inferred on the basis of the assumption that the inner core has the same composition as the surrounding material of the outer core¹. If, however, the observed increase in compressional velocity is related to a compositional change or, as suggested by Elsasser and Isenberg², to a new phase of iron with rearranged electronic orbits, then the inner core might be liquid³. It was believed that conclusive evidence of solidity of the inner core would come from observations of body waves of the type PKJKP, SKJKS or SKJKP, phases that travel through the inner core as a shear wave. But no reliable observations of these phases have been reported so far.

Here we present evidence for solidity of the inner core based on observations of normal modes reported by Dziewonski and Gilbert⁴. The pertinent modes and observed values of their

Table 1 Observed Normal Modes of the Earth sensitive to the Structure of the Inner Core

Mode	Mean period (s)	No. of observations	s.e.m. (s)	Comp. period	UTD124B'—Solid inner core		UTD124B'—Liquid inner core		5.08M		HB ₁	
					Rel. error (%)	Inner core energy Compr. Shear	Comp. period	Rel. error (%)	Comp. period	Rel. error (%)	Comp. period	Rel. error (%)
${}_1S_0$	613.57	11	0.236	614.59	0.17	0.181 0.000	607.39	-1.02	610.06	-0.57	607.4	-1.01
${}_2S_0$	398.54	40	0.084	397.59	-0.24	0.206 0.001	392.31	-1.59	391.42	-1.81	394.0	-1.14
${}_3S_0$	305.84	7	0.129	306.00	0.05	0.233 0.003	301.36	-1.48	301.84	-1.31	300.9	-1.62
${}_4S_0$	243.59	12	0.067	243.80	0.09	0.192 0.007	241.11	-1.03	241.55	-0.84	239.9	-1.51
${}_2S_2$	904.23	21	0.487	904.43	0.02	0.001 0.080	914.94	1.17	917.80	1.50	915.1	1.20
${}_5S_2$	397.36	11	0.157	397.03	-0.09	0.015 0.102	399.93	0.67	398.20	0.21	399.1	0.44
${}_6S_1$	348.41	21	0.046	348.23	-0.05	0.068 0.011	347.10	-0.38	347.38	-0.30	346.6	-0.52
${}_7S_3$	281.37	11	0.113	281.59	0.08	0.004 0.022	282.77	0.50	283.34	0.70	282.1	0.22
${}_8S_1$	272.10	11	0.144	271.79	-0.11	0.115 0.052	271.00	-0.40	270.92	-0.43	270.5	-0.59
Nine modes—r.m.s.					0.12			1.01		1.00		1.02

unique answer, that is, whether it is possible to construct a model with a liquid inner core which would satisfy all the observations.

The attempts to match the data for the modes belonging to group 1 with a liquid inner core model invariably result in a decrease of density in the inner core below the outer core value at the inner-outer core boundary. This result could have been expected, for with relatively well established values for the compressional velocity and zero rigidity the only other way to reduce the bulk modulus is to decrease the density. A reversal of density in this region of the Earth's interior represents an implausible solution. It might be possible to match group 1 data without a density reversal by introducing a strongly super-adiabatic density gradient in the lower outer core to distribute the required decrease in the bulk modulus over a larger volume. But the data of group 2 would remain unsatisfied, and their residuals would in fact increase.

Because the modes of group 2 would have zero energy in the inner core if it were liquid, the only way to increase their eigenfrequencies would be to increase the bulk modulus in the outer core, a condition contradicting that necessary to satisfy the data of group 1. We therefore conclude that solidity of the inner core represents the only solution consistent with the observations of normal modes.

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BIOLOGICAL SCIENCES

Ligandin: a Hepatic Protein which Binds Steroids, Bilirubin, Carcinogens and a Number of Exogenous Organic Anions

THREE protein preparations from the rat liver 100,000g supernatant fraction have been independently purified to homogeneity. They are basic azodye carcinogen-binding protein (β -ABP)^{1,2}, corticosteroid Binder I^{3,4} and Y protein⁵. The compounds they bind include certain steroids and their metabolites, bilirubin and certain carcinogens and their metabolites. Several dyes and cholestyographic agents and other organic anions are also bound (Table 1). Binding occurs whether the compounds are injected *in vivo* or added *in vitro* to liver homogenates.

We now present strong evidence to show that these three proteins are identical. For example, a comparison of their

physical and chemical properties shows a close similarity^{6,7} (Table 2). Furthermore preparations of β -ABP bind bilirubin, bromsulphthalein and 2-hydroxyoestradiol-glutathione conjugate. Immunochemical experiments give further support for identity. Monospecific antibodies to Y protein and Binder I show identity between Y protein, Binder I and β -ABP⁸. Similarly, monospecific antibodies prepared against β -ABP⁹ show a similar identity among these three proteins. We have concluded that we are dealing with the same protein and because of its binding properties have decided to call it "ligandin".

Table 1 Compounds Bound by Ligandin

Compound	Laboratory	Nature of binding
Azodye carcinogen	(K) (L)	Covalent ^{1,6}
Azodye-GSH conjugate	(K)	Noncovalent ⁶
Corticosteroids	(L)	Noncovalent ⁹
Corticosteroid anionic metabolites	(L)	Noncovalent ^{3,4}
Oestradiol metabolites	(L) (K)	Noncovalent ⁹
Bilirubin	(A) (K)	Noncovalent ^{5,8,17-19}
Bromsulphthalein	(A) (K)	Noncovalent ^{5,8,10-12,14,17-19}
Indocyanine green	(A)	Noncovalent ^{5,19}
Phylloerythrin	(A)	Noncovalent ^{5,11}
Iodopanoic acid	(A)	Noncovalent ^{5,11}
Flavaspidic acid	(A)	Noncovalent ^{5,11}
Methylcholanthrene	(L)	Noncovalent ¹⁶
Methylcholanthrene metabolite	(L)	Covalent ¹⁶

K, Ketterer; L, Litwack; A, Arias.

Table 2 Physical and Chemical Properties of Ligandin

$S_{20,w}$	3.5 (K), 3.47 (L), 3.5 (A)
$S^{\circ}_{20,w}$	3.7 (L)
Molecular weight	
(a) Protomer	
Gel filtration	45,000 (K), 44,000 (A), 37,800 (L)
Sedimentation-diffusion	36,000 (L)
Sedimentation-equilibrium	45,900 (K), 42,900 (A)
Amino-acid analysis	37,300 (L)
(b) Subunits	23,000 (K), 22,000 (A)
pH_i (boundary electrophoresis) (isoelectrofocusing)	8.4 (K), 8.7 (A)
	8.9 (L), ~9.0 (K), 8.7 (A)
Amino-acid analysis (approximate)	Asp ₃₄ , Thr ₁₁ , Ser ₁₃ , Glu ₃₉ , Pro ₁₆ , Gly ₁₈ , Ala ₂₆ , Val ₁₇ , Met ₁₄ , Leu ₄₆ , Ileu ₁₉ , Tyr ₁₂ , Phe ₁₇ , Lys ₃₃ , His ₅ , His ₅ , Arg ₂₁ , Try ₂ , Cys ₄ (Similar analyses found by all authors)

K, Ketterer; L, Litwack; A, Arias.

Ligandin is abundant in rat liver and immunochemical titration in one laboratory gave it a value of 4.46% of the total cell sap protein⁸ and, in another, a value of 4%. From binding studies, it also seems to be present in fully developed amphibia, reptiles, birds and mammals including monkey and man, but is absent from gill-breathing vertebrates^{10-12,14}. Its levels are low in the foetal liver, but increase rapidly *post-partum*¹⁰⁻¹⁴, and are also low in hepatomas^{13,15,16}.

Although most abundant in the liver, ligandin has also been identified in the kidney^{8,15}, where it occurs in the tubule cells⁸ and also in mucosal cells of the small intestine⁸. Ligandin constitutes approximately 2% of supernatant protein in homogenates of kidney and small intestinal mucosa and has not been detected in plasma, bile, brain or other tissues⁸.

Ligandin binds a range of endogenous substances non-

covalently but the affinity of binding varies. For example, oestradiol metabolites seem to bind with equal or greater affinity than corticosteroid metabolites⁹, and bromsulphthalein can be displaced by low concentrations of bilirubin but not by bile salts⁵. There is also specificity of binding among carcinogens. For example, whereas azodye carcinogens^{1,2,6} and methylcholanthrene¹⁶ are bound, acetylaminofluorene, a carcinogen with which azodye carcinogens are often compared biochemically, is not bound.

Carcinogens differ from other compounds in that in addition to binding to ligandin noncovalently, they also bind covalently. For example, azodye carcinogens are bound noncovalently as a glutathione conjugate⁶ and are also bound covalently to the protein through the sulphur of a cysteinyl residue¹⁷. Methylcholanthrene also binds both noncovalently and covalently¹⁶.

Compounds which induce drug and steroid metabolizing enzymes of the endoplasmic reticulum (for example, phenobarbitone and DDT) also induce ligandin^{8,18-20}.

In ontogenetic, phylogenetic, induction and competition experiments, there is a direct relationship between the hepatic concentration of ligandin and the net flux of bilirubin, bromsulphthalein and other organic anions between plasma and the liver cell^{5,8,10-12,14,18-20}. These observations support the hypothesis that ligandin is a major determinant of selective hepatic uptake of certain small molecules and may also influence the flux of metabolites from the liver into the plasma or bile.

Where steroids and carcinogens are concerned, however, azodye-binding protein has been detected in the nucleoplasm²¹ and an additional function of ligandin may be to transport compounds which affect cellular differentiation into the nucleus.

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Human Repetitious DNA, Satellite DNA and Chromosome Interarm Fibres

It is of interest that RNA (cRNA) made complementary *in vitro* to human repetitious DNA has been found to hybridize with human chromosomal DNA at the centromeres and at the ends of the arms in light microscope cytological preparations¹. These are the locations where we saw the largest number of DNA-containing interarm fibres in electron micrographs of whole mount human mitotic chromosomes² (Fig. 1).

Previously, mouse satellite DNA, a highly repetitious fraction of mouse DNA³, was also found to be localized at the centromeres in mouse chromosomes hybridized with mouse satellite cRNA^{4,5}. The similarities in chromosome localization and the repetitive nature make the human repetitious DNA found by Arrighi *et al.*¹ a likely candidate for human satellite DNA.

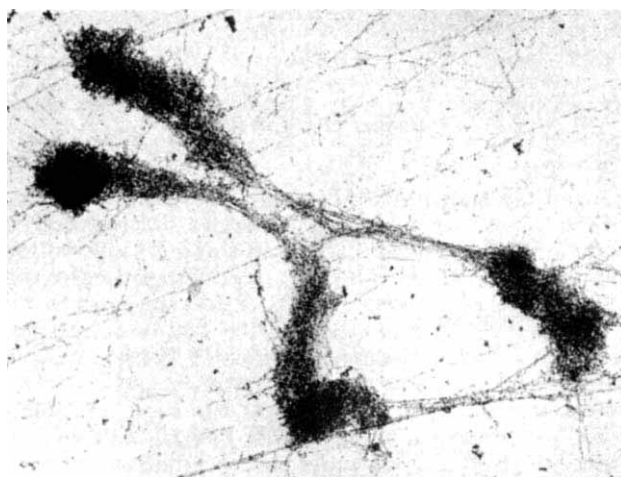


Fig. 1 A human lymphocyte chromosome treated with 25 µg/ml. trypsin (Worthington) showing fibre attachments at the centromere and bottom arms ($\times 6,125$).

In addition to interarm fibres crossing at the centromere and at the ends of arms in human chromosomes, we found fibres crossing all along the length of the sister chromatids in some chromosomes (Fig. 2). These fibres were fewer in number at their crossing sites. The fact that they were not always seen may have been due to the stretching forces exerted on the chromosomes when they were placed on the water surface of a Langmuir trough before being picked up on electron microscope grids. The only interarm fibres which were always seen were those crossing at the centromere.

The interarm fibres found at sites other than the centromere and ends of arms would also have been expected to hybridize

with cRNA to repetitious DNA if they contain repetitious DNA. One reason for this apparent contradiction may be that the DNA template for the complementary RNA did not contain all sequences for the repetitious DNA. Corneo *et al.*⁶ have isolated a second human satellite DNA fraction. Another reason for the apparent lack of hybridization might be the smaller number of fibres and therefore fewer genes involved at these sites. In *Drosophila* salivary gland chromosomes which contain multiple copies of genes Hennig *et al.*⁷ found *Drosophila* satellite DNA in some species distributed all along the chromosome bands, although predominantly located at the kinetochore. They noted that the gene enhancement of polytene chromosomes may have increased the possibility of detecting satellite DNA at sites not detectable in normal chromosomes.

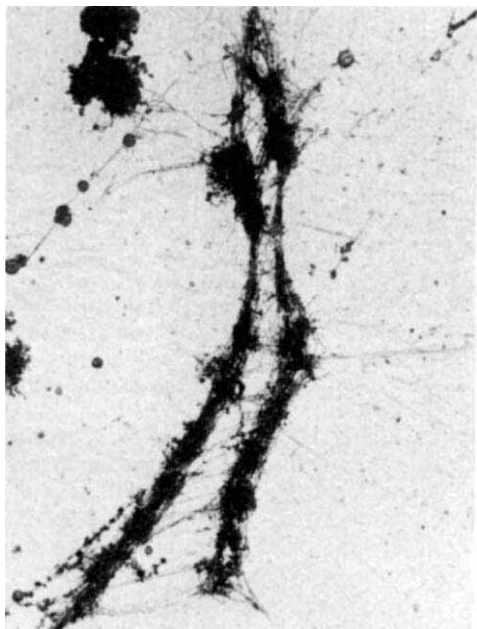


Fig. 2 Relaxation of chromosome with 800 µg/ml. pronase to reveal many interarm fibres crossing all along the length of the two chromatids ($\times 9,100$). (Reproduced by permission of the editor of *J. Cell Biol.*)

Satellite DNA seems to be different from total chromosomal DNA in many ways. First, it bands at a different buoyant density in caesium chloride gradients, showing a different base composition^{8,9}. Second, it is highly repetitious and reassociates rapidly after denaturation¹⁰. Third, it does not seem to code for protein in the mouse, or, as in the case of guinea-pig a satellite, to code for an aberrant protein¹¹. Fourth, Maio and Schildkraut found mouse satellite DNA to be associated with different protein components after extraction of isolated mouse chromosomes with high salt¹². Fifth, it has been found in association with constitutive heterochromatin in mouse cells¹³. Sixth, it has the apparently discrete chromosome localization when hybridized with complementary RNA.

What is a possible role for interarm fibre-satellite DNA? Does it challenge the classic genetic theory of a complete set of chromosomal genes within each chromatid to find genome-carrying fibres running between sister chromatids? If highly repetitious DNA does not code for protein, it may have the structural role proposed by Walker *et al.*³. And that role may be to package genome DNA for delivery to daughter cells.

The mechanism which causes the separation or dissolution of the interarm fibres at cell division remains unknown. Failure of the fibres to separate, however, might cause the two chromatids to be drawn into the same daughter cell. This might result in the phenomenon, trisomy, where three homologous chromosomes instead of two are found at the next metaphase.

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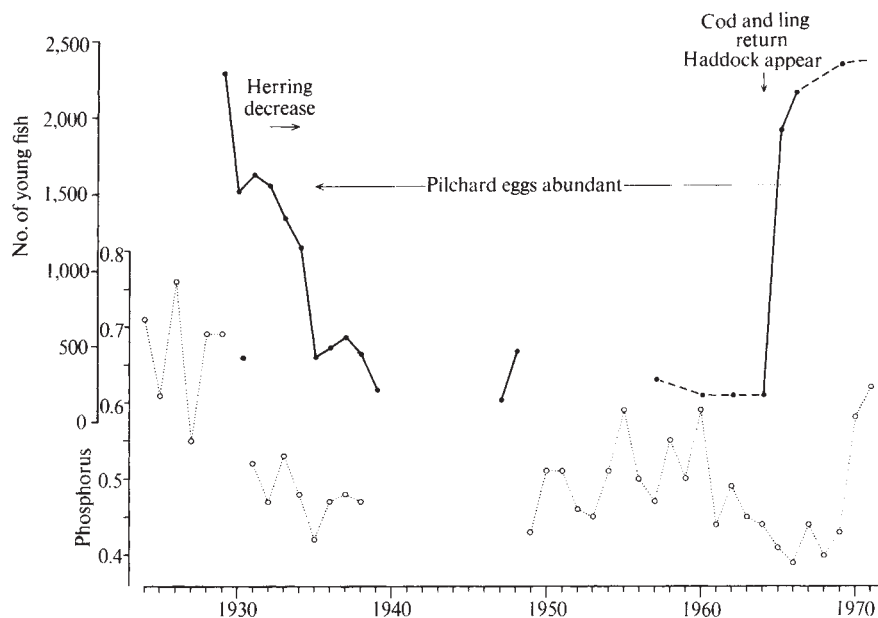
Changes in Biological Conditions in the English Channel off Plymouth during the Last Half Century

COMPARABLE observations on chemical and biological conditions in the western English Channel off Plymouth have been made with varying regularity and intensity since 1921, and regular records of nutrient salts and the larger zooplankton were kept from 1924 to 1939. During that period a notable change took place: the herring fishery, which had been a regular winter feature at Plymouth, petered out in the early 1930s when young herring ceased to join the shoals^{1,2}, and at the same time there was a decrease in the amount of nutrient salts present in the water in the winter³ and a very marked decline in the quantity of zooplankton including the planktonic stages of bottom-living fish. From 1935 pilchard seemed to take the place of herring⁴.

After the Second World War observations were resumed, and since 1965 there has been a return to the conditions which were familiar in the 1920s^{5,6}. Evidence from a number of sources confirms these changes, including observations on nutrient salts, primary productivity, phytoplankton succession and zooplankton abundance, and numbers of young and adult fish. A general summary of some of the events is shown graphically in Fig. 1.

One of the more striking features of the earlier period was the decrease in the maximum winter phosphate values, which fell from an average of 0.67 µg/at. P/l. during 1924–29 to an average of 0.48 during 1931–38. Analyses have been continued regularly^{7,8} apart from the war years and tests have been made to ensure that the results are strictly comparable in spite of changes in methods. In general the low winter phosphate levels prevailed, although three isolated years did show higher values than in 1931–38 (similar annual fluctuations also appear in the period of high winter phosphate levels). Since 1969, however, we have had two consecutive years of high

Fig. 1 Diagram to show (a) sums of average monthly catches of young fish (excluding clupeids) in standard hauls of the 2 m ring trawl off Plymouth (●—●), 1924–29 being shown as the average for those years; (b) winter maxima of inorganic phosphate (○···○) as $\mu\text{g/at. P/l.}$ Changes in occurrence of herring, pilchard and certain gadoids are also given.



winter phosphate, the value of 0.62 found in 1971 being the highest recorded since the "rich" years before 1930, and not far removed from the average for that period.

Measurements of primary production by the ^{14}C method have been made since 1964. Assessments of total annual production in grams of carbon under 1 m^2 sea surface show an increase in recent years as follows: 1965, 140; 1966, 166; 1967, 127; 1968, 188; 1969, 195; 1970, 212.

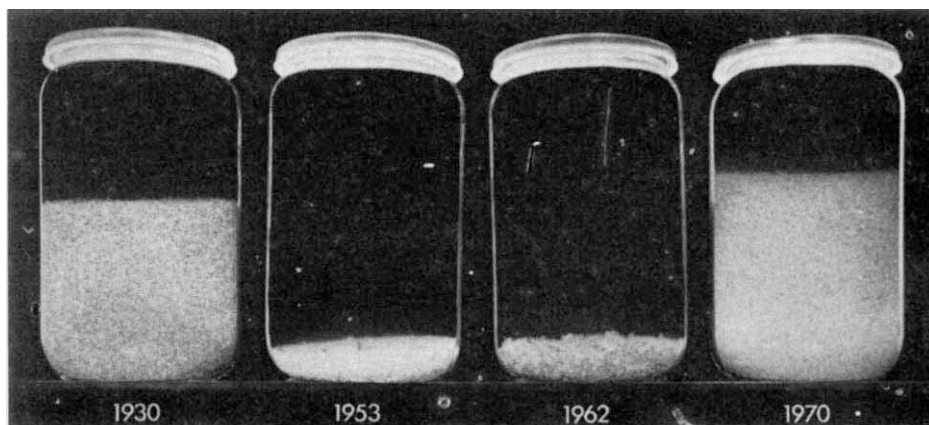
At the same time there has been a steady increase in the spring maximum of phytoplankton since 1967. The diatoms *Biddulphia sinensis* and *Coscinodiscus concinnus* have also become much more abundant in the spring and early summer during the past 4 or 5 yr.

Until 1931 regular zooplankton collections made with the 2 m net (ringtrawl) had been very rich in numbers of animals especially the copepod *Calanus* and larval stages of Crustacea⁹. The samples were also characterized by the occurrence of certain species of medusae, larval worms, echinoderm larvae and euphausiids which disappeared almost completely during the late 1930s. After 1935, however, there was a notable increase in the numbers of eggs and post-larvae of pilchard, especially from April to July. When sampling was resumed in 1945 it was clear that the conditions found in 1935–39 still prevailed. Although the methods had to be modified slightly after the war and although collections were not made so regularly in certain years, a watch was kept on the situation, and a careful series of comparisons was made of the sampling capacity of the nets to ensure continuity of the series. In about 1965 it was evident that a change was taking place, and since then observa-

tions have been intensified. The numbers of the planktonic stages of teleostean fish other than clupeids increased spectacularly and have returned in abundance approximating to that found in the "rich" years before 1930. By 1970 the whole catch of zooplankton had increased greatly; *Calanus* was once more extremely abundant in the late spring (Fig. 2), and other animals such as larval forms of worms and echinoderms and euphausiids which had virtually disappeared in the 1930s became quite frequent again in the catches. Coincident with these changes in young fish and zooplankton there are indications of a reduction in the intensity of spawning of pilchard and a shift of the main season of abundance of pilchard eggs away from the spring and early summer months towards the autumn. There are also some signs that herring may be returning to the area, although not yet in sufficient numbers to support a fishery.

These changes seem to be linked in some way. Plymouth lies close to a well marked boundary between boreal forms and warm water species. Thus the years of paucity of plankton and abundance of pilchard may correspond to a period of general retreat northward of the boreal fauna during the recent amelioration of climate in the northern hemisphere. That boreal forms are now returning during a period of worsening climate is borne out by the evidence for recent increases in numbers of adult fish such as cod and ling, while haddock have appeared for the first time. Additional evidence has been found on the seashore where boreal forms such as *Balanus balanoides* have increased greatly since 1961¹⁰. Much of the initial increase in the non-clupeid young fish which we noted in 1965–66 must

Fig. 2 The relative abundance of larger zooplankton off Plymouth in certain years, as sampled with the 2 m net (about $5,000\text{ M}^3$ of water filtered in a standard haul). The catches are the maxima found each year in late May or June, and are shown settled in jars holding 2 l. The 1953 sample is almost entirely eggs of pilchard, while *Calanus* predominates in the 1930 and 1970 samples. (Photo by D. Nicholson.)



itself have been due to a local increase in the population of adult fish.

A number of theories have been put forward to explain the decrease in nutrient salts and plankton during the 1930s (summarized in ref. 11). Now that conditions are returning to the earlier state there seems little doubt that basically the cycle must be related to climatic change. Warming of the Arctic would affect circulation in the North Sea by allowing Atlantic water to extend farther north, thus reducing the pressure from the north and from the south-west (through the English Channel)¹². Cooling will have the reverse effect and intensify penetration into the North Sea from these directions. At the same time the population centres of northern fish and plankton animals will shift towards the south again.

The whole picture thus results from a combination of factors, namely climatic change, its effects on water movements, and shifting of centres of animal populations. The resulting changes in the ecosystem may also affect the cycle of nutrients⁴. More detailed accounts of different aspects of this research will be published elsewhere.

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Evidence for a Conformational Change in Nerve Membrane with Depolarization

THERE has been much interest in identifying and characterizing the protein elements in cell membranes which seem to be involved in regulating membrane ionic permeability¹⁻³. We now have further evidence that membrane-bound proteins in nerve axons are involved in ion permeability regulation. It was first found that external treatment of isolated crayfish nerve axons with the proteolytic enzymes papain, trypsin, and chymotrypsin (unpublished data) considerably affected the axons, resting permeability to potassium, sodium, and chloride, while having minimal effects on the action potential. It was also found that the external application of 1-fluoro 2,4-dinitro-

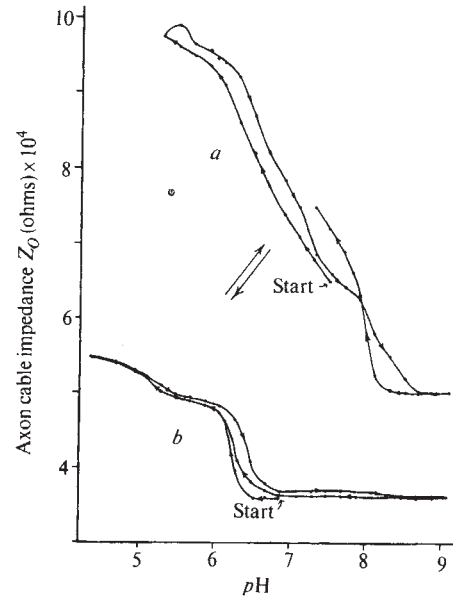


Fig. 2 Impedance of the axon. *a*, Resting axon, $K_o = 5.4$ mM, $V_m = -87$ mV; *b*, potassium depolarized axon, $K_o = 40$ mM, $V_m = -43$ mV.

benzene (which reacts with protein reactive groups) considerably reduced chloride permeability without reducing the resting membrane potential or action potential⁴.

Further information on the permeability role of membrane-bound protein was obtained by measuring the axon cable impedance Z_o as a function of pH, where $Z_o^2 = r_m(r_i + r_o)$. Changes due to pH in the axoplasmic resistance r_i and external resistance r_o would not be expected to occur, for these resistances primarily depend on the free ions potassium, sodium and chloride. Therefore, any change in the axon cable impedance Z_o with pH would primarily reflect those changes which occur in the membrane resistance r_m .

In the experiments described here, the medial giant axon from the ventral nerve cord of crayfish (*Procambarus clarkii*) was dissected from its adjacent axons in saline containing (mM): NaCl 205, KCl 5.4, CaCl₂ 13.5, MgCl₂ 2.6, Tris (hydroxymethyl) aminomethane-maleate buffer 5.0. The pH was adjusted by adding either HCl or NaOH to the saline. The axon was cannulated with a micro-pipette electrode through which constant current pulses ΔI were injected (Fig. 1). From the opposite end of the axon, a potential recording micro-pipette was also inserted to record the membrane potential V_m , and the change in membrane potential, ΔV_m , due to the injected constant current pulses. From these measurements, the axon cable impedance Z_o was determined as $2\Delta V_m/\Delta I$.

Titration of the axon revealed a pH dependency of the axon cable impedance which closely resembled a protein dissociation curve (Fig. 2*a*). Back titration from the pH extremes of 5 and 10 often resulted in hysteresis or completely new curves such as might indicate exposure of new ionizing groups as a result of denaturation. The results from axon to axon showed considerable variability in the absolute magnitude of cable impedance but the regions of impedance transition

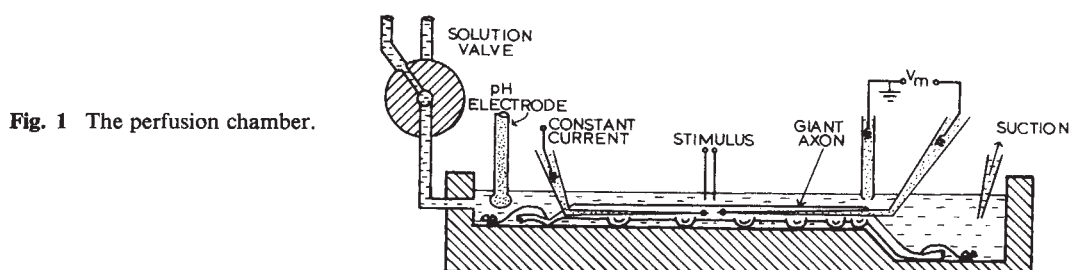


Fig. 1 The perfusion chamber.

with pH remained essentially unaltered. Most noticeable have been transitions in cable impedance around pH 6.3 and pH 8 to 8.5. These are tentatively identified as resulting from histidine and sulphydryl groups.

Experiments were also performed in elevated external potassium solution which depolarized the axon membrane more than enough to produce an action potential if this depolarization were done by voltage stimulating electrodes. This depolarization was also sufficient to produce "sodium inactivation" and render the axon inexcitable. In the potassium depolarized state, the axon cable impedance is less than that of the axon in normal saline and the cable impedance changes with pH show less overall variation (Fig. 2b). Less variation in the data from axon to axon in the depolarized state is obtained. An especially steep portion of the pH dependence curve occurs in the pH range 6 to 6.5. We believe this represents a cooperative transition in the surface membrane protein. This transition probably involves the unmasking of buried histidine residues. The data here suggest that the process of nerve excitation involves a change in the conformation of membrane-bound protein where an increase in the potassium ion concentration in the membrane displaces calcium during depolarization. This potassium-calcium displacement mechanism has often been suggested as a possible mechanism for nerve excitation⁵.

Further evidence here that histidine is involved in excitation comes from photo-oxidation studies utilizing rose Bengal⁶, which, when used to treat the axons, resulted in immediate loss of the action potential and a slower loss of membrane potential on exposure to light. This effect was mitigated when the amount of magnesium present was increased. These results strengthen our view that a conformational transition involving histidine groups of surface membrane proteins is involved in the action potential and an ion permeability regulation.

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X-ray-induced Morphological Differentiation of Mouse Neuroblastoma Cells *in vitro*

THE high radiosensitivity of undifferentiated human neuroblastoma is well known, but the nature of changes induced by irradiation is uncertain. Two ideas have emerged from the observations of radiobiologists and radiotherapists: (a) neuroblastoma cells are destroyed or (b) they are induced to differentiate or "mature" into ganglioneuroma by ionizing radiation. Experimental evidence to support the hypothesis of radiation-induced differentiation is lacking. The availability of mouse neuroblastoma cell culture provides an opportunity to study the nature of cellular alterations produced by ionizing radiation. This paper shows that X-irradiation induced morphological differentiation of neuroblastoma cells which in the presence of serum undergo morphological maturation.

In our laboratory, neuroblastoma cells have been cultured from the primary mouse tumour by the procedure of Dr R. Klebe, of Yale University. Cells have not been cloned. They are grown as a monolayer in the Falcon plastic flask containing F12 medium with 10% agamma globulin newborn calf serum, penicillin (100 µg/ml.) and streptomycin (100 U/ml.) and are maintained at 36° C in a humidified atmosphere of 5% CO₂ in air. When grown in Falcon plastic flasks, the average doubling time of neuroblastoma cells is 24 h. This cell line has acetylcholinesterase but no butyrylcholinesterase activity, indicating its neuronal nature¹. Cells (10⁵) were plated in a T-30 flask and X-irradiated 24 h later. Radiation factors were 250 kVp, 30 mA, with added filtration 0.5 mm Cu and 1.0 mm Al (hvt=1.36 mm Cu). Dose rate was 139 rad/min. The turntable was rotating during exposure. Cells were X-irradiated at room temperature with a single dose (200–1,000 rad) or a fractionated dose (800–1,400 rad, 200 rad/day). The medium in the irradiated cell population turned acidic relatively fast and therefore a frequent change of medium was necessary. For the study of the repair of irradiated neuroblastoma cells, the culture received 400 rad in two fractionated doses. The time interval between two fractions was 4 and 24 h. The irradiated controls received 400 rad in a single dose. The control cells were treated similarly except that no radiation was given.

The formation of axons (cytoplasmic extensions greater than 50 µm long) was regarded as an expression of morphological differentiation, and the enlargement of the nucleus and cell size as indicating morphological maturation. A single-cell suspension of control and irradiated cell population using 0.25% viokase solution for 20 min was prepared and the cells were counted with a haematocytometer. The cells stained with trypan blue (0.1% in saline) were considered dead and subtracted from the total count. The growth inhibition of the neuroblastoma cell population *in vitro* 3 days after irradiation was calculated as follows:

$$\frac{\text{No. of control cells} - \text{No. of irradiated cells}}{\text{No. of control cells}} \times 100$$

The cell viability was also determined after staining the attached cells with trypan blue.

X-irradiation of mouse neuroblastoma cell culture induced axon formation (Fig. 1B, C, and D) as well as enlargement of cellular and nuclear size. A similar change in cellular morphology is known to occur during development of the mammalian nervous system. There were several multinucleated differen-

Table 1 X-ray-induced Morphological Differentiation of Neuroblastoma Cells *in vitro*

Dose	Differentiated cells (% of total cell population) Days after treatment				% viability
	1	2	3	4	
Control	8 ± 1.5*	9 ± 2.5	12 ± 3.0	11 ± 1.6	95 ± 2.0
200 rad	—	—	17 ± 0.9	—	93 ± 2.0
400 rad	—	—	26 ± 3.5	—	83 ± 3.5
600 rad	22 ± 3.9	28 ± 5.9	39 ± 7.7	47 ± 3.4	67 ± 7.0
800 rad	—	—	40 ± 3.5	—	60 ± 6.0
800 rad (200 rad/day)	—	—	29 ± 2.0	—	81 ± 2.7
1,000 rad (200 rad/day)	—	—	27 ± 4.2	—	82 ± 2.7
1,200 rad (200 rad/day)	—	—	30 ± 0.9	—	69 ± 3.5

* Standard deviation.

Cytoplasmic extension greater than 50 µm in length was considered as an axon. Differentiated cells in the fractionated-dose experiment were counted one day after the last fractionated dose. The viability of cells was determined by using supravital stain (trypan blue). The stained cells were considered dead. Attached cells in the dish were first stained with trypan blue and then differentiated cells were scored to avoid the scoring of dead cells while counting the differentiated ones. The % viability was determined 3 days after single dose or 1 day after the last fractionated dose. Each value represents an average of six to ten samples.

tiated cells. The presence of polyploid neurones in the irradiated cell population has recently been confirmed by a higher DNA content per cell¹. It is interesting to note that some mammalian neurones, such as Purkinje cells, are polyploids. Many irradiated cells increased in cellular and nuclear size without forming axons. The degenerative changes in the X-ray-induced differentiated cells were characterized by the appearance of vacuolated cytoplasm (Fig. 1D) and finally fragmentation of nuclear and cytoplasmic materials (Fig. 1E). These cellular changes resemble those observed when matured mammalian neurones are cultured in the absence of nerve growth factors. Almost all cells stained with trypan blue were smaller, indicating that all large cells in the irradiated cell population were viable.

Table 1 shows a quantitative evaluation of differentiated cells as a function of time and dose. The control cell population contained about 8% differentiated cells, whereas the irradiated cell population 24 h after 600 rad contained about 22% differentiated cells. At this time there was no significant cell loss in the irradiated cell population, so that the increase in the differentiated cell population cannot be attributed to the killing of radio-sensitive cells. The maximum increase in the differentiated cell population was seen 3–4 days after irradiation. The number of differentiated cells after a single dose appeared greater than that after a fractionated dose, indicating that dose was more important than the time (Table 1).

X-irradiation of Chinese hamster ovary (CHO) and baby hamster kidney-21 (BHK) cells *in vitro* did not produce morphological changes similar to those of neuroblastoma. CHO and BHK-21 cells were grown in experimental conditions similar to those of neuroblastoma cells.

Morphological maturation was evident as early as 24 h after irradiation, and by the third day most differentiated cells had matured morphologically. In the control cell population only a few of the differentiated cells matured morphologically. Did maturation of cells result from cellular changes produced by X-irradiation or from some "factor" in the serum? To investigate this question, serum-free medium was added immediately after X-irradiation (600 rad). Serum-free medium was also added to a non-irradiated cell population. Many cells in the irradiated and non-irradiated cell population formed relatively shorter axons, but the cellular and nuclear size did not increase. This showed that the presence of serum after X-irradiation was necessary for the expression of maturation. The formation of axon in the non-irradiated neuroblastoma cell population is consistent with an earlier observation². Because of the induction of axons by serum-free medium, we wondered if the X-irradiation of the medium was changing the constituents of the serum in a way that induced differentiation. To investigate this, cells were X-irradiated (600 rad) and fresh growth medium was replaced immediately after exposure. In the irradiated controls, growth medium was not changed. The

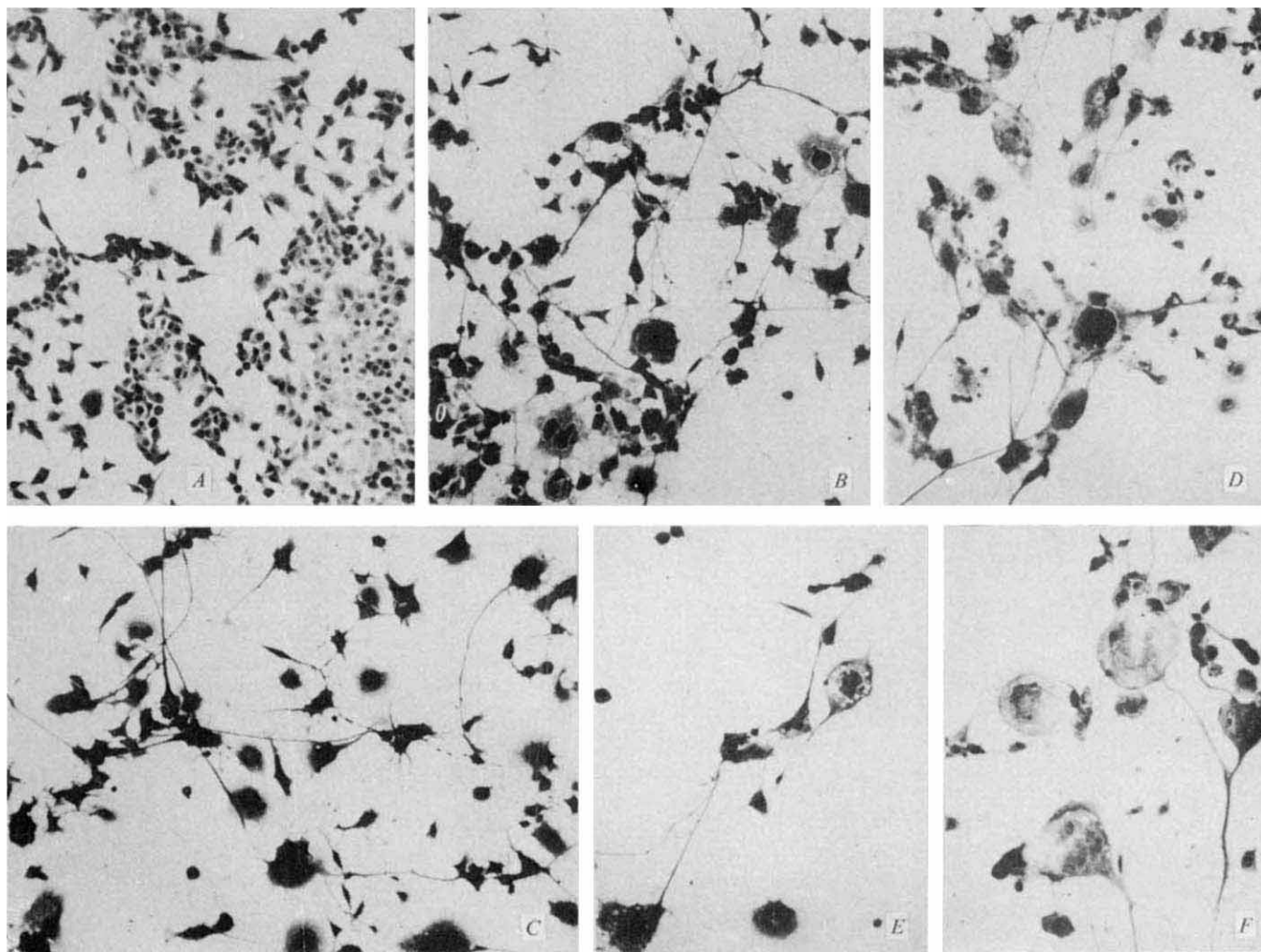


Fig. 1 Photomicrographs of control and irradiated cell population. Cells while attached to the flask were fixed in formaldehyde (1 : 10 dilution of concentrated formaldehyde) for 20–30 s, washed quickly once with distilled water, stained with 0.1% cresyl violet for 2 min, rinsed quickly twice with distilled water and air-dried. Immersion oil (3 ml.) was added to the flask before taking photomicrographs. Non-irradiated cell population (A) showed small, round and undifferentiated cells. Irradiated cell population (B) and (C) received a single dose of 600 rad and 1,000 rad respectively. Many differentiated and matured cells were present 4 days after irradiation. The irradiated cell population (D) was given 1,400 rad (200 rad/day) and stained 24 h after the last dose. Vacuolated cytoplasm (E) and fragmentation of cytoplasmic and nuclear materials (F) in matured neurones were also observed ($\times 56$).

differentiation and maturation in both groups of irradiated cell population occurred in a similar manner. A change in serum constituents was therefore not responsible for the radiation-induced morphological differentiation.

Table 2 Effect of X-irradiation on Mouse Neuroblastoma Cells *in vitro*

Dose (rad)	% inhibition	(Total number of cells $\times 10^4$)
Control	0	106.0 $\pm$ 6.0
200	27.0 $\pm$ 4.0*	81.0 $\pm$ 8.0
400	62.0 $\pm$ 2.0	41.0 $\pm$ 4.6
600	80.0 $\pm$ 1.0	21.7 $\pm$ 0.4
800	92.0 $\pm$ 2.0	9.9 $\pm$ 1.5
1,000	94.0 $\pm$ 1.0	6.6 $\pm$ 0.7
200 + 0 h + 200	66.0 $\pm$ 4.0	37.0 $\pm$ 6.0
200 + 4 h + 200	50.0 $\pm$ 4.0	54.0 $\pm$ 2.9
200 + 24 h + 200	50.0 $\pm$ 6.0	54.0 $\pm$ 3.7
1,200 (200 rad/day)†		35.0 $\pm$ 3.0

* Standard deviation.

† Cells were counted 1 day after the last dose.

Cells were X-irradiated 24 h after plating and counted 3 days after irradiation. In the experiments using fractionated dose schedule, the cell number was counted 3 days after first dose. The % growth inhibition of neuroblastoma cell population was calculated as follows:

$$\frac{\text{No. of control cells} - \text{No. of irradiated cells}}{\text{No. of control cells}} \times 100$$

Each value represented an average of six to eight samples.

To study the recovery of neuroblastoma cells, a fractionated-dose technique³ was used. The recovery was measured on the basis of cell survival. An interval of 4 h between two fractionated doses allowed only slight recovery (Table 2). Increasing the interval produced no further recovery. Our present criteria of measuring recovery assume the resumption of cell division after recovery; but X-irradiation induces differentiation and differentiated neuroblastoma cells would not resume cell division. Thus even though radiation damage may have been fully repaired, this cannot be measured on these criteria.

Vinblastine sulphate, which is known to interfere with the assembly of microtubules, inhibits the induction of axon formation by serum-free medium², dibutyl adenosine 3' 5' cyclic monophosphate⁴ and 5-bromodeoxyuridine⁵. When vinblastine sulphate (2.43×10^{-9} M) was added to the neuroblastoma culture immediately after X-irradiation, examination of the cells 3 days later showed that in this case also axon formation was inhibited.

X-irradiation inhibited the growth of neuroblastoma cells and this effect was dose-dependent (Table 2). Radiation is known to inhibit cell division, and so one may argue that inhibition of cell division is mandatory for morphological differentiation. The fact that serum-free medium blocks cell multiplication and induces axon formation in neuroblastoma cells² tends to support this contention. We have recently found, however, that dopamine, 6-hydroxydopamine⁶ or DL-glyceraldehyde (Sakamoto and K. N. P., in preparation) inhibits cell division but does not induce morphological differentiation. The inhibition of cell multiplication is therefore not sufficient for morphological differentiation.

From these results it seems not only that X-irradiation induces differentiation of neuroblastoma cells, but that it accelerates maturation of differentiated cells. Moreover, acetylcholinesterase activity in cells induced to differentiate by X-irradiation increases by a factor of ten¹. The effects of vinblastine sulphate on induction of differentiation suggest that the effect of X-irradiation may depend on its promoting the assembly of microtubules.

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Fertility Reduced in a Caged Native House Fly Strain by the Introduction of Strains bearing Heterozygous Chromosomal Translocations

GENETIC manipulation of insect pests¹ is an attractive method of control because of its specificity. The numerous possible types of manipulations fit into two categories: (1) the release of sterile insects that affect only the succeeding generation and (2) the release of partially sterile or fully fertile insects that carry inherited factors which will affect several subsequent generations. The first category includes sterile males², hybrid sterility and cytoplasmic incompatibility³. The second category includes chromosomal translocations, other gross chromosomal abnormalities, recessive lethal mutations, sex-limited lethal mutations, detrimental and conditional lethal mutations, and a combination of meiotic drive plus detrimental or lethal factors.

So far most genetic manipulations have been of the first sort, but studies of genetic manipulations in the second category are in progress and we have been studying chromosomal translocations in the house fly, *Musca domestica* L. Translocations, when heterozygous, reduce fertility because a regular proportion of the products of meiosis (eggs and sperm) are deficient for chromosomal parts required for life (viability). Larval and pupal mortality is also increased in some cases. Several investigators have discussed the use of chromosomal translocations for suppressing insect populations^{4,5}. Using X-rays, we induced more than 300 chromosomal translocations in house flies and we analysed each strain for egg hatch (fertility), transmission of the translocation, and sex ratios of progeny. Table 1 shows the results of this analysis for three strains that had a low fertility when the heterozygous translocation-bearing males were outcrossed with appropriately marked non-translocation-bearing virgin females. This procedure was practical

Table 1 Egg Hatch, Transmission of the Translocation, and Sex Ratio of Progeny from Three Strains of House Flies bearing Chromosomal Translocations involving Three Chromosomes

Translocation	Hatch (%)	Progeny bearing translocation (%)	Males (%)
A*	33.5	53.2	52.8
B*	30.7	48.0	51.0
C†	27.3	72.0	72.0

* Reciprocal translocation between three autosomes.

† Reciprocal translocation between the Y chromosome and two autosomes.

Males bearing the translocation were crossed to appropriately marked, non-translocation-bearing females.

because no crossing over occurs in the male house fly⁶. Two involved the reciprocal translocation of three autosomes (translocations A and B) and one involved the reciprocal translocation of the Y chromosome and two autosomes (translocation C).

For the tests, translocation-bearing males or males plus females of individual strains were placed in laboratory cages at one or two ratios (9 : 1 or 4 : 1) with virgin wild type males and females so the reduction of fertility (fertility coefficient) of the resulting matings could be assessed. The fertilities of F_1 , F_2 , and F_3 sib crosses with or without the addition of further heterozygous translocation-bearing flies were also assessed and the accumulated fertility coefficients (fertility compared with controls) were determined. Equal numbers of females were maintained in test and control cages. Also equal numbers of test and control larvae were allowed to develop in the same volume of medium.

The fertility and accumulated fertility coefficients were determined in the following way (C. F. Curtis, personal communication). Controls = 100% fertility. The fertility coefficient of the treated population = % fertility of the treated population compared to controls in generation 1. The accumulated fertility coefficient of the treated population in generation 2 = % fertility of the treated population compared to controls $\times$ the fertility coefficient of the treated population in generation 1, and so on.

Table 2 shows the lowered fertility obtained in the tests based on the reduction in egg hatch and on larval and pupal mortality compared with control populations containing wild type flies. With translocations A and B, the fertility coefficient ranged from 10.3 to 21.6% of controls in the first cross and the accumulated fertility coefficient through as many as four generations ranged from 1.0 to 6.4%. With translocation C, the lowered fertility ranged from 42.0 to 45.8% in the first cross and the accumulated fertility coefficient through four generations was 34.5%. The translocations involving three autosomes were more efficient at reducing fertilities than was the translocation in which the Y chromosome and two autosomes were reciprocally translocated.

We emphasize that population control, not eradication, is the goal of our research. If the greatest reduction in fertility we obtained in one generation, 89.7%, were maintained, it should hold a fly population static throughout the season or cause it to decrease if the reproductive potential is less than ten times and density-dependent factors are not operating. Thus the number of flies released plus the decreasing number of wild flies combined could be smaller than the wild population, even with fairly effective insecticide control. Results such as these could be realized in the field, and control of insect pests might be achieved, or the method might enhance control in combination measures.

The tests reported here are at best only simulations of natural circumstances. Flies for release (bearing the translocation) had to be separated from the non-translocation-bearing progeny of appropriate genetic crosses, and while the methods are operable in the laboratory, refinements would have to be introduced into the system to make such methods practicable for massive field releases. Small field trials are possible using the methods reported here, however, and have been undertaken at three sites in Florida during 1969 and 1970. The results of these tests will be reported later.

One refinement of the system includes the release of insects bearing homozygous rather than heterozygous translocations⁷. This is especially important when the insect stage released is itself the pest. Homozygous strains can be reared without selection, so large numbers are more easily obtained. Also, the maximum reduction in fertility that can be achieved by the release of homozygous translocations (up to 75%) is realized when the translocation chromosomes are 1 : 1 with the normal homologues in a population. Thus, fully competitive insects need be released only in a 1 : 1 ratio with the native insects, but to obtain more than a 90% reduction in fertility each genera-

tion, several strains bearing different translocations in the homozygous form would need to be utilized. We have obtained three homozygous translocation-bearing strains of house flies in fifteen attempts. All three cases involve the simplest form of reciprocal chromosomal rearrangement which includes just two chromosomes. Such translocations in the heterozygous form do not yield the required reduction in fertility necessary to reduce house fly populations. We must obtain homozygous translocation strains where more chromosomes are involved in the rearrangement and thus a greater reduction in fertility is obtained. We have twenty such strains in the heterozygous form. The final requirement is that the strains selected for control must be competitive when introduced into the native environment. Therefore, several strains will have to be

Table 2 History of the Reduced Fertility achieved by Crosses between Male House Flies or Males plus Females bearing Heterozygous Translocations and Wild Type Flies (All Cages contained a Minimum of 200 Flies)

Cross	Fertility coefficient (%)	Accumulated fertility coefficient (%)
Control—wild type $\times$ wild type (for three generations)	100.0	100.0
9 Trans A $\text{♀} \times$ 9 Trans A $\text{♂} \times$ 1 wild type $\text{♀} \times$ 1 wild type ♂	10.3	10.3
$F_1 \times F_1$	30.3	3.1
9 Trans A $\text{♀} \times$ 9 Trans A $\text{♂} \times$ 1 $F_1 \text{♀} \times$ 1 $F_1 \text{♂}$	29.2	3.0
$F_2 \times F_2$	36.1	1.1
$F_2 \times F_2$ (no further additions)	34.2	1.0
Based on the % hatch of 17,223 total eggs and the emergence of 3,902 total adult flies.		
Control—wild type $\times$ wild type (for four generations)	100.0	100.0
9 Trans B $\text{♀} \times$ 9 Trans B $\text{♂} \times$ 1 wild type $\text{♀} \times$ 1 wild type ♂	20.5	20.5
$F_1 \times F_1$	53.7	11.0
Added 9 Trans B $\text{♀} \times$ 9 Trans B $\text{♂} \times$ 1 $F_1 \text{♀} \times$ 1 $F_1 \text{♂}$	29.8	6.1
$F_2 \times F_2$	67.8	7.5
$F_2 \times F_2$ (no further additions)	36.0	2.2
$F_3 \times F_3$	86.0	6.4
$F_3 \times F_3$	63.1	1.4
Based on the % hatch of 14,217 total eggs and the emergence of 1,659 total adult flies.		
Control—wild type $\times$ wild type (three generations)	100.0	100.0
9 Trans A $\text{♂} \times$ 1 wild type $\text{♀} \times$ 1 wild type ♂	17.9	17.9
9 Trans A $\text{♀} \times$ 9 Trans A $\text{♂} \times$ 1 wild type $\text{♀} \times$ 1 wild type ♂	21.6	21.6
Added 9 Trans A $\text{♂} \times$ 1 $F_1 \text{♀} \times$ 1 $F_1 \text{♂}$	34.1	6.1
$F_1 \times F_1$	31.4	6.8
Added 9 Trans A $\text{♀} \times$ 9 Trans A $\text{♂} \times$ 1 $F_1 \text{♀} \times$ 1 $F_1 \text{♂}$	23.6	5.1
$F_2 \times F_2$ (no further additions)	56.3	3.4
$F_2 \times F_2$	37.9	2.6
$F_2 \times F_2$ (no further additions)	32.5	1.7
Based on the % hatch of 10,761 total eggs and the emergence of 1,102 total adult flies.		
Control—wild type $\times$ wild type (one generation)	100.0	100.0
9 Trans C $\text{♂} \times$ 1 wild type $\text{♀} \times$ 1 wild type ♂	45.8	45.8
Control wild type $\times$ wild type (four generations)	100.0	100.0
4 Trans C $\text{♂} \times$ 1 wild type $\text{♀} \times$ 1 wild type ♂	42.0	42.0
$F_1 \times F_1$	82.2	34.5
Added 4 Trans C $\text{♂} \times$ 1 $F_1 \text{♀} \times$ 1 $F_1 \text{♂}$	83.2	34.9
$F_2 \times F_2$	None	34.5
$F_2 \times F_2$ (no further additions)	None	34.9
$F_3 \times F_3$	None	34.5
$F_3 \times F_3$	None	34.9
Based on the % hatch of 8,833 total eggs and the emergence of 3,259 total adult flies.		

prepared so that the most viable in a particular natural setting can be selected.

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is unresolved until more substantial data are available to indicate its distance from *PGM₁* and *6PGD*.)

If this ordering with *Rh* near the centre of the group is correct, the *Rh* : *PGM₁* interval is estimated as 0.27 M (95% probability limits are 0.16 and 0.43) and it is thus the longest map distance to be well estimated directly in man. As the data come from one laboratory, it is likely that direct estimations of longer intervals—up to 0.35 M or beyond—are within the limits of the method when data from several laboratories are combined. The simple addition of the lengths of the two component intervals gives a reasonable estimate of a still longer interval length—*PGM₁* : *6PGD* (over 0.50 M), but this is indirect. Direct estimation of the corresponding recombination fraction may give some indication of the strengths of genetical interference in man but a good estimate would require an enormous body of high quality data.

Table 1 Lods for Various Values of Recombination Fractions

	Recombination fraction						
	0.5	0.45	0.4	0.3	0.2	0.1	0
<i>6PGD</i> : <i>Rh</i> ⁴	0	1.322	2.722	5.023	5.335	0.602	−∞
<i>Rh</i> : <i>PGM₁</i>	0	0.704	1.409	2.947	2.290	−5.370	−∞
<i>6PGD</i> : <i>PGM₁</i>	0	0.016	0.032	0.099	0.053	−0.367	−∞

Lods are standard likelihoods in logarithmic form to base 10.

Pedigrees: CADDY, CAFFN, CALMS, CAOOD, CAPEA, CAPEB, CATTS, CAWWR, CDKGK, CD1LL, CD2BL, CLACL, CLAGE, EA1ME, EA1MR, EB1BR, EB1GS, EL1RS, JM1MY, JN1AN, MX1BK, MX1IE, NP1W, NP1X, NP1AB, NP1AC, NP1U1, NP1U2, NP1U3, PE1DY, PE1LK, PE1MD, PE1PD, PE1YY, SSEDL, SSEGE, SSEGR, SSEMD, SSESE, SY1MD, TY3VA, TY4BE, TY4PN, V21AN, WM1ME, WM1WE, 9J.GN, 9JATN, 9JCMY, 9JTF1, 9JTF2.

In summary, pedigree methods suggest that *Rh* and *El₁* lie between *PGM₁* and *6PGD*. The necessary analyses might have been long delayed but for the hybrid-cell demonstration of synteny of *PGM₁* and *6PGD*. *PeC*, the locus for peptidase C, has recently been added to this syntenic group⁸.

A different hybrid clone, one between human and mouse cells, was used by Conover and Hirschhorn⁹ to assign tentatively this syntenic group or, more exactly, one of its members—the *PGM₁* locus—to a C group chromosome. Fluorescence techniques from Caspersen's laboratory and other new staining procedures should allow this C group chromosome to be more precisely identified.

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The Rhesus Syntenic Group in Man

THE two principal approaches to mapping human autosomes—the study of pedigrees and of cell hybrids—may sometimes complement each other.

Pedigree studies by Lawler and her co-workers¹ established a close linkage (0.03 morgans) between *Rh*, the rhesus set of blood group loci, and one of the loci, *El₁*, that can lead to elliptocytosis of the red cell. There seems to be, in addition, at least one isophenic, or mimicking, locus² which is not closely linked to *Rh*. Weitkamp, Guttormsen and Greendyke^{3,4} have demonstrated that *Rh* is linked to *6PGD*, the polymorphic locus for 6-phosphogluconate dehydrogenase. The maximum-probability estimate of the map distance (taking no account of the sex difference uncovered by these authors) is about 0.24 morgans (0.13–0.36). The study of man/hamster cell hybrids has recently added *PGM₁*, the locus for one species of phosphoglucomutase molecules, to this syntenic group^{5,6}. A high correlation was found in loss or retention of the human *PGM₁* and human *6PGD* isozymes as studied in clones derived at various stages during the progressive loss of whole chromosomes (preponderantly human) from the hybrid cells.

To determine the sequence of the *PGM₁*, *Rh*, *6PGD* loci, thus shown to be on the same chromosome, I have estimated the map distances on a large section of the data tested during 1965–68 in my laboratory (then in Glasgow).

A set of aggregate lods (standardized likelihoods in logarithmic form) is given in Table 1 for the *Rh* : *PGM₁* interval together with another set for the *PGM₁* : *6PGD* interval. The lods of Weitkamp *et al.*⁴ are used unmodified for the *Rh* : *6PGD* interval and no account has been taken here of a sex difference in the recombination fractions. From inspection alone, it would seem that *Rh* lies between the other two loci, and a full analysis, using the Bayesian methods of Renwick and Bolling⁷, confirms this. The posterior odds on this ordering as opposed to any alternative, are 5 : 1. (The relative position of *El₁*

Alpha Rhythms in the Hyperkinetic Child

HYPERKINESIS is a poorly understood but common disorder of childhood the main features of which are excessive motor activity, a short span of attention, low tolerance of frustration and hyperexcitability, often resulting in learning difficulties and a poor motor performance even where intelligence is normal. The neurophysiological substrate of this disorder remains undefined and emotional factors have often been invoked as its sole basis. Some children with this syndrome improve when given central nervous system stimulant drugs; We have attempted to distinguish those children who are likely to improve from those who may not.

The reaction times of human subjects are shorter when the signal to respond is given during spontaneous beta waves than when it is given during spontaneous alpha waves¹. Beta waves seem to represent a process of cortical excitation, such that they appear when stimuli are perceived as novel and disappear with habituation²⁻⁴. Alpha rhythms are said to represent an inhibitory process involving discharge of sub-cortical influences back into the cortex, and effecting the termination of cortical excitation associated with automatization or habituation of learned behaviour².

Alpha rhythms were therefore studied in a group of hyperkinetic children as a possible measure of increased attention span. Twenty-eight children between the ages of 5 and 12, recognized by history and examination to have hyperkinesis and decreased attention span without psychomotor retardation, were chosen. Subjective scoring by parents and school teachers and objective measurements of activity level, impulsivity, performance and coordination were obtained for each child at the beginning of the study. Intravenous dextroamphetamine (10 mg m⁻² body surface area), methylphenidate (20 mg m⁻² body surface area), or saline in equivalent volume was injected in random order, using double blind methods, into these twenty-eight children during an electroencephalographic (EEG) examination. The EEG responses were collected on magnetic tape 15 min before and 15 min after the injection, for 3 min each. Five control subjects were injected with one of the stimulant drugs (four had dextroamphetamine and one had methylphenidate), and their EEG responses were similarly recorded on magnetic tape. Silver electrodes were applied using the International 10-20 System and the eyes of the subjects were kept open throughout the experiment⁵. The potentials between P₄ and O₂ were analysed. A PDP-12 computer was used for the data analysis. The computer sampled four 17 s segments each from pre-injection and post-injection periods and performed the Cooley-Tukey fast Fourier transformation. The statistical test used the mean power of these four segments for pre-injection and post-injection periods.

Each of these children was put on oral therapy with dextroamphetamine, methylphenidate, or a placebo in random order, using double blind methods. Repeat subjective scoring and objective measurements of activity level, impulsivity, performance, and coordination were obtained at the end of a 3-week period. At that time all the subjects of the study were put on oral therapy with one of the central nervous system stimulant drugs (dextroamphetamine or methylphenidate) and followed during the next 2 yr.

Table 1 shows the correlations between response to oral therapy with central nervous stimulant drugs and the response of EEG alpha rhythms to the intravenous injection of these drugs. The final results are expressed by a ratio r , where $r =$

EEG power alpha range (8 to 12 Hz) in the post-injection period
EEG power in alpha range (8 to 12 Hz) in the pre-injection period

The results generally show an increase in alpha activity in those who responded well to oral drug therapy but not in those who did not. The Mann-Whitney U test was used to examine

the difference between these two groups, which differed significantly ($P < 0.05$). The control subjects injected with one of the central nervous stimulant drugs showed a consistent increase in alpha activity.

A retrospective analysis of the clinical effects noted immediately after injection of one of the drugs showed that those children who had a poor response to the oral therapy on follow-up had all shown a remarkable amount of restlessness, talkativeness, and inability to sleep.

Table 1 Response of Hyperkinetic Children to Central Nervous System Stimulants

	Drug injection		Saline injection	
	Hyperactive children			
	Good response to the treatment	Poor response to the treatment	Normal controls	Hyperactive children
r values of the subjects	28.85	1.54	6.16	2.33
	8.20	0.68	2.77	1.27
	6.60	0.43	1.86	0.98
	5.13	0.42	1.45	0.98
	3.66	0.40	1.36	0.81
	1.85	0.13		0.78
	1.61			0.55
	1.49			
	1.32			
	0.76			
	0.74			
	0.49			
	0.48			
	0.40			
	0.37			

The basic neurophysiological defect in hyperkinesis of childhood seems to be a disorder of inhibitory mechanisms in the central nervous system such that irrelevant stimuli are not filtered out and the child is at the mercy of all the stimuli in his environment. Central nervous stimulant drugs seem to be able to activate or strengthen these inhibitory mechanisms. If alpha rhythm in the EEG is an index of relaxed wakefulness, habituation to a class of stimuli, should result in augmentation of alpha. The increase in alpha rhythms seen in those who responded well to the drugs supports this hypothesis.

This work was done while I was a research fellow in neurology at the Harvard University Medical School in Boston.

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Loss of the Sensory Process of an Insect Receptor at Ecdysis

CAMPANIFORM sensilla on cockroach legs are mechanosensitive sense organs associated with the exoskeleton which are thought to help coordinate the animal's walking movements¹. Although the fine structure of campaniform sensilla has been elucidated²,

little is known about the morphogenetic changes they undergo during moulting. I wish to report initial findings of an electron microscopic investigation of the development of cockroach campaniform sensilla.

Each campaniform sensillum functions through a single primary sense cell—a large bipolar neurone with a cell body, of epidermal origin, which is in a pocket of endocuticle. Its axon goes directly to the central nervous system, but the dendrite is associated with the cuticle and has a tip containing a basal body which gives rise to a highly modified cilium. The distal portion of this, the sensory process, inserts into a thin cap of cuticle which is the site of mechanical stimulation². The sensory process is a membrane-limited bundle of 350–1,000 parallel microtubules, which play a role in mechano-electric transduction in campaniform sensilla^{3,4}. The intimate physical association of the sensory process with the cuticular cap of the sensillum raises the question of the fate of the sensory process at ecdysis.

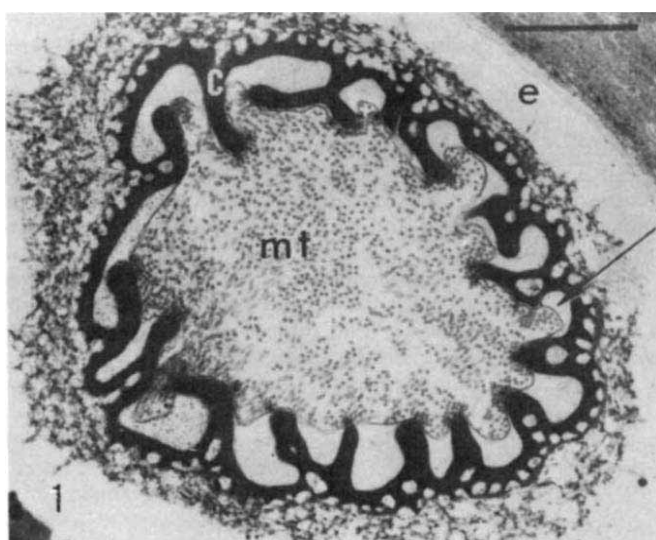


Fig. 1 Cross-section through a sensory process within an exuvium of the cockroach *Blaberus giganteus*. e, Extracellular space; c, cuticular sheath; mt, microtubules; arrow indicates cell membrane, which appears intact ($\times 17,000$). Scale = 1 μ m.

Fig. 1 provides clear evidence that the sensory process is shed at ecdysis, after which the cuticular sheath, plasma membrane, and 1,350 microtubules are present and appear intact (compare with Fig. 12, ref. 2). The microtubules, however, which are parallel *in vivo*, do not have uniformly cross-sectioned profiles in Fig. 1, suggesting that their proximal ends were floating free in the detached sensory process.

Fig. 2 is a cross-section through the tip of a moulted sensory process—the point at which it inserts into the cap of cuticle covering the sensillum which is displaced during mechanical stimulation. The electron-image of the moulted sensory process tip is indistinguishable from that of an intact campaniform sensillum (compare with Fig. 10 of ref. 2). The cell membrane, encompassed by an extracellular cuticular sheath, encloses a tightly packed bundle of more than 700 microtubules. The cross-sectioned microtubules are surrounded by dense, amorphous material which may participate in microtubule maintenance and initiation². Although the cytoplasmic matrix seems to have left the sensory process (Fig. 1), the dense material remains at its tip, indicating a close association between the microtubules and the dense, amorphous material which surrounds their walls.

Fig. 3 shows a cross-section through a moulted sensory process which is apparently degenerating. The sensory process has greatly decreased in volume and occupies only a small

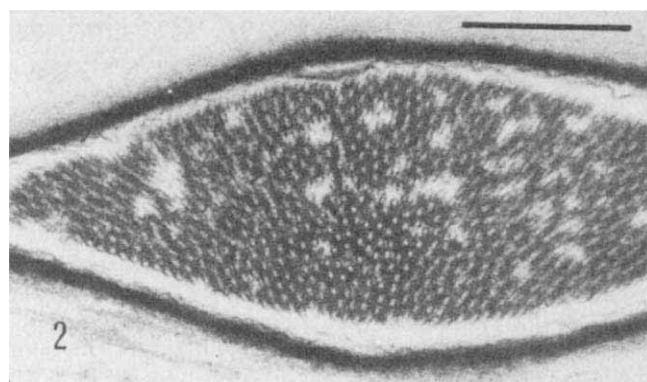


Fig. 2 Cross-section through the distal tip of a sensory process within an exuvium. Dense, amorphous material surrounds the microtubule walls ($\times 44,000$). Scale = 0.5 μ m.

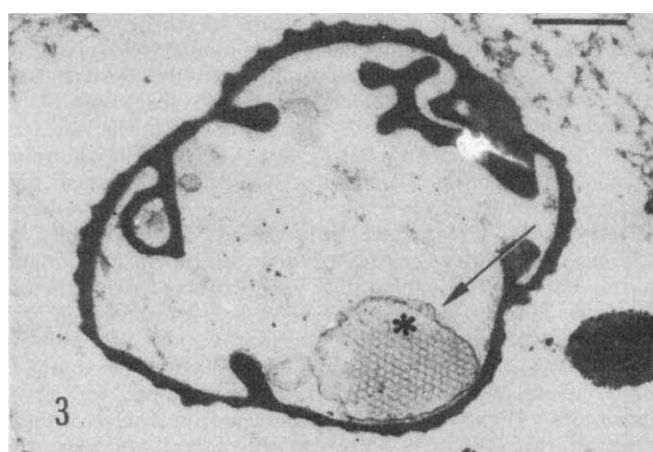


Fig. 3 Cross-section through a degenerating sensory process within an exuvium. Arrow indicates cell membrane. Microtubules have reorganized their structure into a crystalline body (asterisk) ($\times 25,000$). Scale = 0.5 μ m.

portion of the space defined by the cuticular sheath. The microtubules have undergone rearrangement into a crystalline body which is reminiscent of crystals formed by the interaction of microtubule protein with vinblastine sulphate⁵.

The loss of the sensory process of the campaniform sensillum at ecdysis is of considerable physiological significance especially when one considers that the microtubule-packed sensory process may be the transducer^{3,4}. Before ecdysis, the cockroach has two cuticles. The old exocuticle, which will be shed, surrounds the new unsclerotized exocuticle, recently synthesized by underlying epidermal cells. At some time before the moult, the old sensory process must become non-functional, and the campaniform sensillum must establish a new functional connexion with the new cuticle.

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Thysanoessa inermis and *T. longicaudata* (Euphausiidae) as First Intermediate Hosts of *Anisakis* sp. (Nematoda: Ascaridata) in the Northern North Sea, to the North of Scotland and at Faroe

REPORTS^{1,2} that live larvae of the nematode *Anisakis* from raw or inadequately cooked fish may cause eosinophilic granuloma of the alimentary tract in man have stimulated research into the ecology, taxonomy, physiology and pathogenesis of this genus in fish (natural second intermediate hosts), marine mammals (natural final hosts) and man and laboratory animals (abnormal hosts)^{3,4}. But little attention has been paid to earlier stages in the life cycle or to a search for first intermediate hosts. A knowledge of early developmental stages of *Anisakis* might contribute to an understanding of geographical and other variations in abundance of the larvae in economically important fish. Uspenskaya⁵ reported larval *Anisakis* in one of 855 specimens of the amphipod *Caprella septentrionalis* Krøyer, 1838, in one of 990 specimens of the decapod *Hyas araneus* (L.) and in one of an unspecified number of euphausiids *Thysanoessa raschii* (M. Sars, 1864) from the Barents Sea. Oshima *et al.*⁶ found five larvae in 3,247 specimens of *T. raschii* and *T. longipes* Brandt, 1851, collected in the northern North Pacific Ocean and Bering Sea. Kagei⁷, according to Gibson⁴, mentioned *T. inermis* (Krøyer, 1846) as a first intermediate host of *Anisakis*.

Anisakis sp. larvae have been found in two species of *Thysanoessa* from the northern North Sea, north of Scotland and at Faroe. The euphausiids were extracted from routine plankton samples collected in 1969 and preserved in 4% formaldehyde on the fishery research vessels Scotia (now Scarba) and Explorer. A total of 2,730 specimens of *T. inermis* from twenty-seven localities are examined for larval nematodes. Eighteen of 1,335 specimens from twelve of these localities (Fig. 1) were infected with larval *Anisakis*, the incidence of infection at individual localities ranging from 0.5 to 4.0%. Examination of 950 specimens of *T. longicaudata* (Krøyer, 1846) from seven localities revealed infection in three of 335 specimens from two localities (Fig. 1), the incidence being 0.7 and 1.0%. This seems to be the first published report of larval *Anisakis* in *T. longicaudata* and also the first published record of them in any invertebrate in these waters.

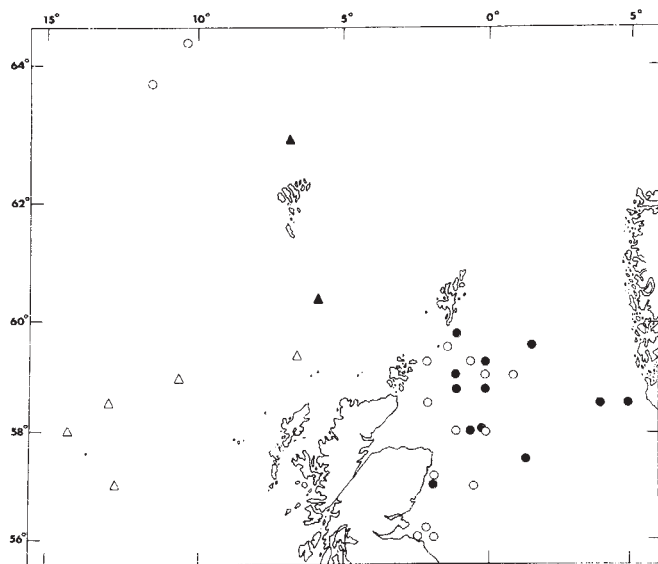


Fig. 1 Chart showing localities at which *Thysanoessa inermis* and *T. longicaudata* were collected and the distribution of infection with larval *Anisakis*. ●, Infected *Thysanoessa inermis*; ○, uninfected *T. inermis*; ▲, infected *T. longicaudata*; △, uninfected *T. longicaudata*.

Only one larva was found in each infected euphausiid; in preserved hosts the larva lay coiled in the haemocoel of the thorax, occasionally with the head or tail extending into the abdominal haemocoel. Observations on the behaviour of larval *Anisakis* in live euphausiids have not yet been possible. The larvae, probably second stage, were from about 5.1 to about 20.6 mm long and morphologically resembled third-stage larvae from fish, designated *Anisakis* sp. larva (1) by Berland⁸. Free-living second-stage larvae of *Anisakis* are comparatively small; such larvae experimentally hatched from eggs in sea water were ensheathed and about 0.22 to about 0.29 mm long (about 0.33 to about 0.37 mm long including the sheath). These probably exsheath after being eaten by euphausiids and grow to the lengths reported above; no euphausiid has so far been seen with larvae less than about 5 mm long. Larval *Anisakis* cannot be specifically identified as yet. There may be only the one species in the areas considered here, possibly *A. simplex* as defined by Davey³.

Larval *Anisakis* were not seen in other euphausiids (463 specimens of *T. raschii*, 500 of *Nyctiphanes couchii* (Bell, 1853) and 488 of *Meganyctiphanes norvegica* (M. Sars, 1857)) collected at some of the localities where infected *T. inermis* or *T. longicaudata* were found. However, Dr P. van Banning (private communication) found an *Anisakis* larva, about 19.0 mm long, in one of 3,178 specimens of *M. norvegica* collected in 1970 from the northern North Sea.

Further work is necessary before any assessment can be made of the relative importance of *Thysanoessa* species, euphausiids in general and of other invertebrates in the life-cycle of *Anisakis*.

The only other larval nematodes seen during the present survey were *Contracaecum* sp.⁸; they occurred singly in some *T. raschii* and *N. couchii* (and in some chaetognaths *Sagitta elegans* Verrill, 1873) from the inshore locality off the north-east coast of Scotland where *T. inermis* infected with larval *Anisakis* were found (Fig. 1).

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Archaeopteryx Again

HEPTONSTALL¹ believes that my estimate of the mass of *Archaeopteryx*^{2,3} (200 g) is far too light, and that a figure of 500 g is more likely.

He derives the figure of 500 g by comparing the linear dimensions of the bones of *Archaeopteryx* and *Columba livia*, and using the reconstruction by Heilmann⁴ to suggest the body size. I should like to criticize this comparison for a number of reasons.

First, Heptonstall suggests that the average mass of *Columba livia* is 400 g, a value derived from Pennycuik⁵, but this species seems to be rather variable in size, for Hartman⁶ quotes the

mean mass of six male specimens as 307 g and a female as 278 g, while Spector⁷ gives the mean of four specimens as 270 g. There is therefore no particular justification in using a mean mass of 400 g for *Columba livia*, and it may well be that Pennycuik's animals were very large specimens. Equally, it is not known whether the skeletons of *Columba* which match in size that of *Archaeopteryx* come from large or small *Columba*.

Second, it is questionable whether *Columba* is a very good choice for comparison with *Archaeopteryx*. Pigeons have a higher proportion of their mass as flight muscle than other birds. On average, flight muscles contribute 17% of the mass in other birds, but about 25% of the mass in pigeons^{6,8}. Further, the bones of *Archaeopteryx* and of jackdaws, *Corvus monedula*, can be matched for size as well as with *Columba livia*. The jackdaw is an insectivorous species, and is arboreal, so might be a reasonable comparison with *Archaeopteryx*. Greenewalt⁸ suggests that this species weighs about 220 g, though to be fair one can be no more sure that this was the weight of the particular specimens whose skeletons are available than one could in the case of the pigeons.

A third point of disagreement concerns the reconstruction of *Archaeopteryx* by Heilmann⁴. Heptonstall has sent me the measurements he used and comparing these with a cast of the Berlin specimen of *Archaeopteryx* suggests that the body form shown in the reconstruction is much too deep dorso-ventrally, by perhaps 30%, and is also too long by 20%. I therefore remain convinced that a mass of about 200 g is a reasonable guess for *Archaeopteryx*³.

Furthermore, Heptonstall's¹ reply to the comments by Bramwell⁹ on the use of the formula

$$v = \sqrt{\frac{2L}{C_L \rho A}}$$

is seriously misleading. He says that if C_L and A are fixed, and L is set at its maximum value, then v is given its maximal value. This would be true of a fixed wing aircraft, but birds are not in this category. Pennycuik¹⁰ has calculated for gliding pigeons a range of C_L from around 0.3 at high speeds to 1.3 at low speeds (and, indeed, as high as 2.8 when the pigeon is hovering); these changes in C_L are brought about by extending or partially folding the wings, thus altering A . Very similar results have been obtained with falcons¹¹. (These values of C_L obtained by Pennycuik were calculated with the areas of the body strip and tail included in the figure for the wing area¹.) The velocity which Heptonstall originally calculated¹² is, then, the maximum diving speed with the wings fully extended, a rather improbable configuration. Out of interest, I recalculated the flying speeds using the same formula. For the "minimum" flying speed, $V_{\min}$, in level flight, I assumed that the wing was fully extended, and used a figure of 1.3 for C_L (based on Pennycuik's data¹⁰), while for the "maximum" flying speed $V_{\max}$, I assumed the lower figure of 0.3 for C_L and a wing area reduced by 30%, both of these figures also based on the performances of Pennycuik's pigeons¹⁰. The estimate of the mass of 200 g was used; the "wing area" included the body strip and the tail. These suggest a speed of 6.3 m/s for $V_{\min}$ and 15.3 m/s for $V_{\max}$. However, it must be emphasized that, quite apart from their highly speculative nature, these are not likely to have been the real minimum or maximum flying speeds. Stalling speed is likely to have been rather lower than the estimated $V_{\min}$, while a few wing beats might have slowed the bird even further, if it was trying to land rather than keep flying. Equally, a diving *Archaeopteryx* might well have achieved higher speeds than the estimated $V_{\max}$, if it had folded its wings more than I have suggested.

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DR HEPTONSTALL writes: I agree with Yalden that before comparing specimens of a living species with *Archaeopteryx* with a view to estimating the live weight of the latter, one must know both the weight and the skeletal dimensions of the living specimens. This relationship in Pennycuik's specimens of *Columba livia* is available from his paper⁵ as he supplies the dimensions of the wing bones. As a result of recent correspondence on this subject^{2,9} I have made a biometrical analysis of a specimen of the crow *Corvus corone corone* which weighed 553 g. Swinton¹³ states that *Archaeopteryx* was similar in size to the larger common species, namely the raven, but this does not seem to be correct. In general my measurements indicate that the carrion crow was a little larger than *Archaeopteryx* and suggest a weight of 450–500 g for the latter.

My use¹² of the equation relating velocity to lift and wing area appears to require further clarification. I used this equation to determine the minimum radii of horizontal and vertical flight paths, these being determined by the maximum lift which the wings were permitted to generate. If we consider a horizontal circular path radius r with angle of bank θ , it follows from resolving the forces that

$$L \cos \theta = mg \quad (i)$$

$$\text{and } L \sin \theta = \frac{mv^2}{r} \quad (ii)$$

On fixing L at $2mg$ the banking angle is found from (i) to be 60° . Combining (ii) with the equation quoted by Yalden and eliminating v gives

$$r = \frac{2m}{2C_L \rho A \sin \theta}$$

As θ is fixed at 60° and m and ρ are constants, r is inversely proportional to the lift coefficient times the wing area. It follows therefore that for paths of minimum radius both C_L and A must have maximum values. My use of the term "maximum velocity"^{11,12} was intended to apply only to these conditions. It is, of course, obvious that a higher velocity could have been reached on flight paths of greater radii if the wings were reduced in area and/or the lift coefficient was lowered. On this point I should like to correct a statement made by Yalden in which he says that "changes in C_L are brought about by extending or partially folding the wings, thus altering A ". The lift coefficient is not determined by wing area but depends on the geometry of the wing and to some extent on the Reynolds number. The most convenient method of changing C_L is to vary the inclination of the wing relative to the airflow.

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Alteration of Sites on the Mammalian Sperm Surface following Capacitation

THE ovum cannot be penetrated by a spermatozoon until the latter undergoes a change termed capacitation¹ which, in the golden hamster, can be produced *in vitro* by incubation in various body fluids²⁻⁴ including serum⁵ and β -glucuronidase⁶.

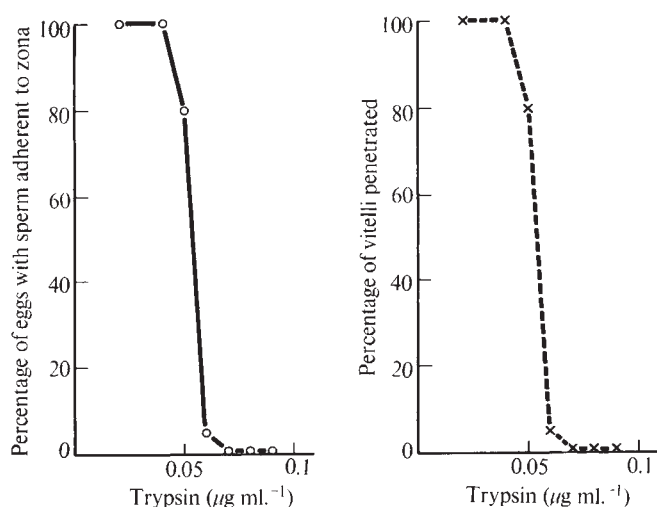


Fig. 1 Washed eggs were placed in 20 μ l. drops of phosphate buffered saline with or without enzyme under oil at 37° C on a rocker in the CO₂ incubator for 1 h. Eggs were washed in 199 M before adding 20 μ l. drops of capacitated sperm at a concentration of $1-3 \times 10^7$ motile sperm ml.⁻¹. After 90 min eggs were washed and scored as stated in the text. Twenty eggs were scored for each trypsin concentration.

The process involves vesiculation of the plasma membrane with the outer acrosomal membrane⁷, which in turn seems to release egg-penetrating enzymes and exposes a surface which can fuse with the vitelline membrane of the egg⁸. As part of a series of investigations on the nature of these surface changes we have studied the effect of proteolytic enzymes on the adherence of sperm to eggs *in vitro*. Our results show that adherence of sperm to the zona pellucida is species specific both before and after capacitation. Capacitation alters the sperm surface, however, so that it will no longer adhere to trypsin-treated eggs.

Epididymal sperm from the golden hamster (*Mesocricetus auratus*) were collected in 199 M as already described⁹ and capacitated by adding 20 μ l. sperm suspension ($1-3 \times 10^7$ motile sperm ml.⁻¹), under mineral oil, to equal volumes of 199 M, containing the contents of an oviduct (17 h after treatment with human chorionic gonadotrophin (HCG), with eggs in cumulus). The culture was incubated at 37° C on a rocker (five to six oscillations min⁻¹) for 5 h. These conditions consistently yielded capacitated sperm. Ova which had been introduced by the oviduct contents were removed and other ova (20–22 h after HCG), free of cumulus, were then added to

each drop. Cumulus was removed with hyaluronidase (450 USP units ml.⁻¹ of Dulbecco's phosphate buffered saline containing 1% polyvinylpyrrolidone). After the various incubation procedures, eggs were washed three times in 199 M, transferred to a slide, compressed slightly beneath a 'Vaseline'-edged cover glass and examined with a phase contrast microscope for adherence of sperm to the zona pellucida and penetration of sperm into the vitellus. To study sperm adherence to the isolated zona pellucida it was removed mechanically by a method described previously for mouse eggs¹⁰.

The effect of trypsin (Crystalline, 196 U mg⁻¹, Worthington) and α -chymotrypsin (3 $\times$ crystalline, Nutritional Biochemicals Co.) on the interaction of capacitated sperm and whole eggs was determined as follows. Eggs were treated with enzyme, washed and exposed to capacitated sperm. As little as 0.05 μ g trypsin ml.⁻¹ reduced sperm adherence and sperm penetration. The decline in adherence and penetration was sharp; both were eliminated at 0.06–0.09 μ g trypsin ml.⁻¹ (Fig. 1). This experiment was conducted four times with similar results. That the block to adherence was due to enzymatic action was shown by the fact that pretreatment of eggs with 1 μ g ml.⁻¹ trypsin plus 10 μ g ml.⁻¹ soybean trypsin inhibitor for 2 h had no effect on adherence and penetration. α -Chymotrypsin also inhibited adherence and penetration, but at a higher concentration (0.2 μ g–0.7 μ g ml.⁻¹). Fig. 1 shows that the decline in penetration parallels the decline in adherence, suggesting that the former is due to the latter.

Table 1 Effect of Treating Isolated Zonae Pellucidae with Trypsin and Chymotrypsin on Subsequent Adherence of Capacitated Sperm

Treatment		Proportion of zonae with adhering sperm	
		Experiment 1	Experiment 2
None Trypsin		20/20 (100%)	13/13 (100%)
	0.10 μ g ml. ⁻¹	3/15 (20%)	—
	0.20 μ g ml. ⁻¹	0/15 (0%)	0/16 (0%)
	0.50 μ g ml. ⁻¹	0/20 (0%)	0/20 (0%)
α -Chymotrypsin	0.7 μ g ml. ⁻¹	3/11 (27%)	0/10 (0%)
	1.0 μ g ml. ⁻¹	0/7 (0%)	—

Eggs were exposed to phosphate buffered saline with or without enzyme for 60 min before sperm exposure.

To determine whether the entire egg or only the zona pellucida was involved, isolated zonae pellucidae were placed in phosphate buffered saline containing either trypsin or α -chymotrypsin. After 2 h the zonae were washed and exposed to sperm. The results (Table 1 and Fig. 2) show that capacitated sperm adhered to untreated zonae pellucidae but not to

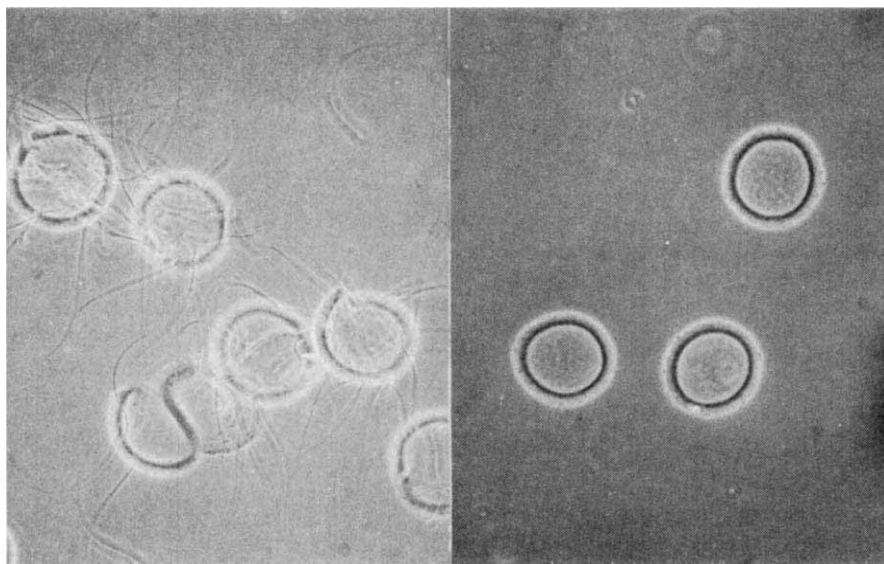


Fig. 2 Effect of exposing isolated zonae pellucidae to trypsin (1 mg ml.⁻¹) for 60 min on subsequent adherence of capacitated sperm. Left: control; right: trypsin-treated. (Phase contrast, $\times 120$.)

those treated with trypsin. Thus it is clear that trypsin and α -chymotrypsin acted directly on the zona pellucida and that the effect was not mediated by the vitellus. This rules out any possibility that the block is produced by the release of cortical granules such as occurs in the zona reaction¹¹. In another experiment the isolated vitelli (eggs without zonae pellucidae) were treated with trypsin and chymotrypsin for 1 hour and then exposed to sperm. Neither enzyme prevented capacitated sperm from entering the vitellus. These experiments show that there are sites for capacitated sperm-adherence on the zona pellucida which are extremely sensitive to trypsin and α -chymotrypsin and that sites on the vitellus are insensitive to these enzymes. The adherence of uncapacitated sperm, however, was unaffected by exposure of eggs to as much as 5 μ g trypsin ml.⁻¹, a concentration about one hundred times greater than that which blocked the adherence of capacitated sperm. Because capacitated sperm preparations contained oviduct contents and uncapacitated sperm preparations did not, we examined the adherence between eggs and capacitated sperm which had been washed free of these materials by centrifugation. Such sperm behaved as if oviduct contents were present: they adhered to untreated ova but not to those previously treated with trypsin.

Many spermatozoa which had not previously been exposed to oviduct contents (zero time) adhered to the ova (Fig. 3). When sperm were exposed to oviduct contents for 60 min there was no adherence (Fig. 4). This was also true of sperm which were exposed to oviduct contents for similar periods and washed by centrifugation before they were combined with eggs. The sperm did not begin to adhere again until they had been in oviduct contents for 3 h (Fig. 4). Sperm left on eggs for 90 min also began to penetrate at 3 hours. Thus restoration of the ability to adhere appears to be linked with the sperm's ability to penetrate. By 5 h and frequently as early as 4 h, capacitation was completed as judged by penetration of virtually all eggs.

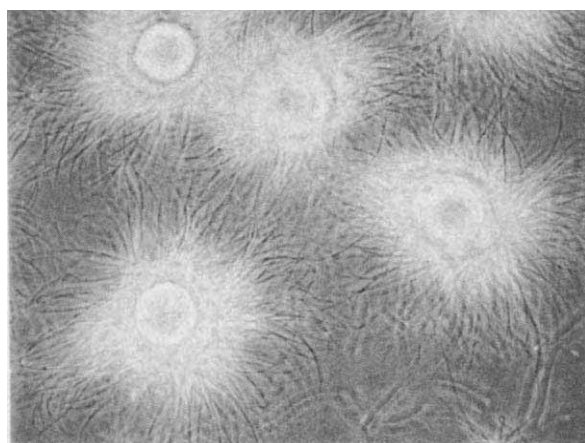


Fig. 3 Uncapacitated hamster sperm adhering to hamster ova. (Phase contrast, $\times 120$.)

It is generally assumed that the sperm of one mammalian species will not fertilize the eggs of another, but little or nothing is known of the role of adherence in this specificity. We found that uncapacitated rat and mouse sperm failed to adhere to the zona pellucida of hamster ova. Capacitated mouse sperm, or capacitated hamster sperm, failed to adhere to the zona pellucida of hamster and mouse eggs, respectively. Species specific receptor sites must therefore be required for adherence of the sperm to the egg either before or after capacitation.

In our research on the nature of sperm adherence to the mammalian egg we are examining sperm glycoproteins, which belong to a class of substances known to be involved in a variety of "recognition" systems, for example, in the "homing"

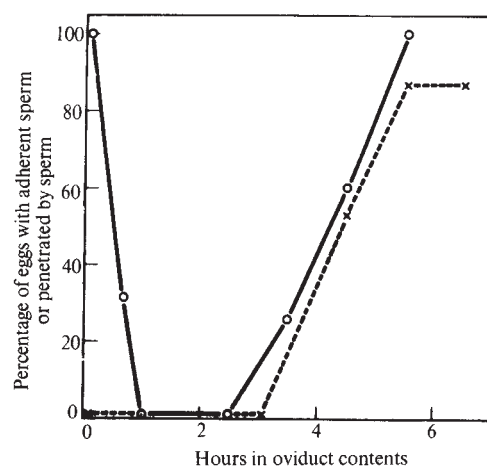


Fig. 4 Sperm were exposed to oviduct contents as described in text and ova were added at intervals. The adherence of sperm to these ova (○) and their penetration by sperm (×) were scored 30 and 90 min later, respectively.

of lymphocytes¹²; as components of viral¹³ and phytohaemagglutinin¹⁴ receptors and in determining antigenic specificity¹⁵. Unlike the adherence of capacitated sperm to the zona pellucida, adherence to the isolated vitellus was unaffected by trypsin. This observation demonstrates clearly that the sperm use yet other adherence sites, or a modification of the same sites, to establish contact with the vitellus. Whether species specificity is involved has not yet been determined.

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Urination as a Social Response in Mice

SOCIAL identification in mice appears to be based largely on the odour complex presented by an animal. The exact nature, as well as the relative importance and signal function of its components—metabolic, glandular and environmental—are still being investigated¹, but their efficacy as olfactory cues is reduced by applying scent to an animal² or by dabbing a mouse with the urine of another, which tends to elicit from conspecifics social responses in accordance with the "urine label"³⁻⁵, at least as far as fighting or exemption from it is concerned. This suggests that the odour of urine present in some quantity overrides other olfactory cues, and that there may be a relation-

ship between social behaviour and the extent of concurrent elimination. Although the active use of urination in territorial marking has long been known⁶ and its occurrence in "open field" tests, where it is generally regarded as a fear response or an index of "emotionality", has received much experimental study, less is known about its incidence and import in social contexts, and the experiments reported here—carried out as part of a study of social sniffing—are an attempt to examine this point.

Eight male and eight female albino mice (TO strain), aged 6 months, and the same number of C57Bl mice, aged 10 months, were used. All were housed singly in 'Perspex' cages (30 × 12 × 12 cm) with absorbent flooring, which was periodically overlaid with a sheet of blotting paper fitting the cage floor. This was usually shredded up into nesting material, and no urination onto it was observed before the experiments during 3 min following its insertion. Social experience was provided two weeks before the first experiment by placing all mice of each strain into a 'Perspex' container (42.5 × 23 × 30 cm) with blotting paper flooring for two 30-min periods, because preliminary studies had suggested the existence of sex-typic sniffing and urinary responses, and these might be more evident in animals having some contact experience with conspecifics of both sexes. In order to make such experience comparable for all animals, any incipient fighting or mounting during these "socials" was immediately checked by gently separating the animals concerned. In four experiments carried out at intervals of 1–2 weeks, each animal's eliminative behaviour during a 3-min encounter with one male and one female partner was then recorded under four conditions: (1) as cage-owner presented with a male "visitor" (O + M); (2) as cage-owner presented with a female "visitor" (O + F); (3) as "visitor" placed in the home cage of a male (V + M); (4) as "visitor" placed in the home cage of a female (V + F). A balanced design was used throughout, equal numbers of animals starting in each condition, with location alternating and pair type arranged in ABBA sequence over 4 successive days. Urination bouts perceived as discrete events were recorded on a multi-channel event recorder and noted separately by a second observer. The quantity of urine passed was assessed by using, in each test, a fresh sheet of blotting paper secured to the cage floor by a narrow 'Perspex' frame, and subsequently measuring the area wetted by superimposing a cm² grid. The home cages used for testing were fitted with a temporary cover with a central

opening through which "visitors" were lowered by hand. No urination was observed during such handling. In experiments 1 and 2, social interactions (sniffing, grooming, fighting and mounting) were simultaneously recorded; the results of these observations will be reported separately. In experiments 3 and 4 (with different pairs) a 3/8 inch wiremesh partition was inserted to keep the partners separated (the C57Bl mice could not be tested against each other in any other way, because fur clipping rather than dye-marking had to be used for their individual identification and this did not make them sufficiently discriminable in free interaction). Partition and hands were carefully washed between trials. Results in respect of urination during the tests are shown in Table 1.

Urination in these encounters is unlikely to have been a fear response, for it was generally most copious in male–female pairs where no fighting occurred, scanty in female–female pairs, and, among males, more abundant in cage-owners than in "visitors", though males were more often attacked in the latter role. Males urinated appreciably more in the presence of a female than in the presence of a male, even though the joint urine product of male–female pairs may be less than that of male–male pairs (experiment 4, Table 1b) because of the very small contribution of the female to the joint total; in the screen experiments (3 and 4) this was only 13% of the total for the pair. Females likewise tended to urinate more in the presence of a partner of the opposite sex, though at a much lower level. Placing a partition between the animals made no difference to this pattern. Though the C57Bl mice were much less responsive, in terms of urination as well as of social interaction, probably because of their age, they showed a similar trend.

The results, confirmed in current replications, suggest that urination, notably in male mice, may occur as a social response, involving sexual identification of the partner, which does not require close body contact. Urination may thus play an adaptive part in social encounters, being relatively ample in the presence of a partner of the opposite sex where continued proximity is of biological advantage; variable among males where drawing olfactory attention to himself would be unfavourable to a newcomer but advantageous to an animal on its home ground; and least among females where neither proximity nor spacing is required. It might thereby exert an effect on population density, group structure and individual interactions similar to

Table 1 Urination as a Social Response

(a) Mean urination bouts of male and female mice during 3-min encounter with another male (M) or female (F) in home cage (O) or partner's cage (V)

Experiment	Strains of S and partner	Males				Females			
		O + M	V + M	O + F	V + F	O + M	V + M	O + F	V + F
1	TO/TO	1.00	0.87	2.01	2.18*	1.00	1.31	0.62	0.56
2	TO/C57Bl	1.25	2.87	5.87	6.50†	1.87	0.82	0.37	0.50
	C57Bl/TO	0.37	0.37	2.00	1.87	1.37	1.25	0.37	0.00
3	TO/TO (screen)	3.00	2.00	6.00	8.00†	0.50	1.62	0.25	0.37
4	C57Bl/C57Bl (screen)	2.00	0.75	2.00	4.12	0.37	0.00	0.00	0.00

* $P < 0.01$. † $P < 0.005$. Friedman two-way analysis of variance.

(b) Amount of urine passed by same-sexed and opposite-sexed pairs during 3-min encounters (mean cm² of floor area wetted)

Experiment	Male/male pairs	Male/female pairs	Female/female pairs
1	9.81	17.40	5.44§
2	8.87	13.15	3.00‡
3	15.37	19.88	0.50§
4	5.75	4.50	0.00‡

‡ $P < 0.025$. § $P < 0.005$. Kruskal-Wallis one-way analysis of variance.

(c) Amount of urine passed by individual male and female mice confronting same-sexed and opposite-sexed partner (experiments 3 and 4; mean cm² area wetted)

	When cage-owner	When "visitor"
Male confronting male	8.12	2.43 ¶
Male confronting female	10.17	11.06¶
Female confronting male	0.90	2.25
Female confronting female	0.12	0.12

|| $P < 0.01$ (two-tailed). ¶ $P < 0.005$ (one-tailed). Wilcoxon matched-pairs signed-ranks test.

that ascribed to agonistic behaviour⁷, but with an additional positive role among opposite-sexed individuals which might correspond to attraction mechanisms of hormonal origin noted in primates^{8,9}, as well as those based on specialized scent glands¹⁰, and could therefore serve as the starting point for the well-known "primer" effects of male odour upon female endocrine function¹¹⁻¹³.

The origin of such differential urination patterns could lie in mother-pup interactions, for females promote urination in pups by licking¹⁴, and opposite-sexed offspring might learn to make different urinary responses to adult female odour. Relationships of an analogous kind have, indeed, been postulated for human psycho-sexual responses¹⁵. Among the more immediate causal factors may be perineal sniffing, which was significantly more prolonged, and preceded urination more often, in opposite-sexed pairs than in same-sexed pairs. The deployment of this eliminative behaviour is likely to vary according to age, social experience and probably other factors, and some of these aspects are now being investigated.

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Hierarchy of Water and Energy Turnover of Desert Mammals

MAMMALS inhabiting desert regions might be expected to have similar physiology, particularly in rates of water use. Groups of ungulates in the field, however, show a wide range of water consumption. We measured, over periods of weeks, the uninhibited intake of water and food by groups of animals grazing together on the same territory and this was integrated with metabolic water from oxidation of hydrogen. Using this approach¹ we found that buffalo (*Bos bubalus bubalis*) have the highest water turnover of any unstressed animal so far measured—200 ml./kg/24 h. *Bos taurus* in the same tropical climate were next in rank order, then *Bibos banteng* and the least water-demanding of the cattle group were *Bos indicus* (123 ml./kg/24 h). In the summer deserts of Australia and northern Kenya, the highest water turnover rates occurred in cattle, with sheep at about half their rate, goats somewhat less and camels (*Camelus dromedarius*) least in their water requirements, at half to one-third the rate of cattle². In temperate environments the same rank order of genera and species was sustained. Because of different degrees of fatness (body solids, Table 1) it is useful to rank the animals by mol/l.^{0.82}, which relates to the number of molecules passing through the size-adjusted volumes of body water.

Antelope grazing with cattle on the Athi River plains of Kenya also showed a hierarchy of water turnovers (Table 1). Eland (*Taurotragus oryx*) and boran (*B. indicus*) cattle were equal in water demands, while kongoni (*Alcelaphus buselaphus*) and wildebeest (*Connochaetes gnou*) are comparable with sheep, at half the rate of eland, and an oryx (*Oryx gazella*) used water at a lower rate than camels.

Table 1 Content and Turnover of Water in Ruminants from Arid Arctic to Arid Tropics

		Body solids % body weight	ml./kg/ 24 h	Water turnover mol/kg/ 24 h	mol/ l. ^{0.82} /24 h
Arid subtropics (summer)					
<i>Bos taurus</i>	(6)	20.7 ± 4.83	161 ± 9.1	8.90 ± 0.5	29.1
Sheep	(7)	43.2 ± 3.36	102 ± 11.6	5.06 ± 0.64	18.7
(Ogaden fat-tail)					
Camel	(3)	28.5 ± 4.47	82 ± 4.9	4.56 ± 0.26	18.2
Equatorial arid (July)					
<i>Bos indicus</i>	(5)	28.0 ± 2.14	75 ± 4.9	4.16 ± 0.26	16.2
Eland	(5)	20.6 ± 2.15	79 ± 14.8	4.40 ± 0.82	14.4
Wildebeest	(1)	27.1	54	3.00	9.6
Kongoni	(2)	18.3	52	2.90	7.05
Arctic (April)					
Reindeer	(6)	23.6 ± 5.25	128 ± 29.7	7.1 ± 1.6	20.2
Moose	(1)	23.0	111	6.20	19.6
Sheep	(5)	37.9 ± 5.57	62 ± 16.1	3.46 ± 0.9	11.2
(Corriedale)					
Goats	(3)	33.0 ± 4.46	52 ± 5.4	2.90 ± 0.3	8.7
Musk oxen	(2)	33.5	35	1.90	7.9

Each group of animals was exposed to the same food supply and environmental conditions, but these differed between groups. The fatness of the animals is greater the more body solids present. Means ± s.d.

The cold desert of Alaska, with precipitation only 50–250 mm/yr, was also investigated. There, reindeer (*Rangifer tarandus*) had rates of water turnover as high as cattle, the moose (*Alces alces*) was somewhat lower but twice the rate of sheep and goats, while musk oxen (*Ovibos moschatus*) turned over water as slowly as camels (Table 1). Each region therefore showed a hierarchy of functional types so that animals differing three-fold in water demands inhabited the same dry areas. Survival times without water during summer in hot desert conditions are inversely proportional to water turnover: 3–5 days for cattle, for sheep 6–10 days and among camels 15–20 days³.

A similar relationship of water turnover to survival time was found for desert dasyurid marsupials (Table 2). *Sminthopsis crassicaudata* turns over water at about three times the rate of *Dasyercus cristicauda* or *Dasyuroides byrnei*, but *Sminthopsis* dies in 3–4 days without water, while *Dasyercus* and *Dasyuroides* without water live indefinitely on insects or fresh meat. *Dasyercus* is centred on the arid inland but its range overlaps that of *Sminthopsis* which is most commonly found near wetter coastal areas. Rain forest dasyurids like *Antechinus* have high water turnover rates above 14 mol/kg^{0.82}/24 h. While resting at 25° C these species turn over about 5 mol of water for 1 mol of oxygen, but *Sminthopsis* uses over twice the water and oxygen turned over daily by *Dasyercus* per metabolic kilogram (kg^{0.82} for water and kg^{0.75} for oxygen). The desert rodents, *Pseudomys australis*, *Notomys cervinus* and *N. alexis*, use 3 mol water to 1 mol oxygen at 25° C but the rate of consumption of both these metabolites is between that of *Sminthopsis* and *Dasyercus*. These findings suggested a linkage between energy use and water turnover. It seemed also that animals evolved functionally for one environment might live successfully when displaced to the desert, although survival under deprivation would be short compared with types naturally selected in the desert. Functional organization like that

of the camel presumably persists for millions of years in spite of environmental changes.

The metabolic rate or oxygen turnover of bovids when related⁵ to $\text{kg}^{0.75}$ follows the same rank order as water turnover. *B. taurus* has a high metabolism (90–110 kcalories/ $\text{kg}^{0.75}/24$ h), then comes *B. indicus* (80–90 kcalories), sheep (60 kcalories), goats⁵, with camels⁶ (50 kcalories/ $\text{kg}^{0.75}/24$ h) lowest in rate of energy use. Among antelopes, the eland (110 kcalories) has a high metabolic rate like cattle but kongoni are lower (80 kcalories) in the range of sheep. Reindeer (102 kcalories/ $\text{kg}^{0.75}/24$ h) also have high rates of oxygen consumption like cattle⁷, but the rate of musk oxen is not known. Thus the ranking for water and oxygen turnover follows the same hierarchy, in each group and environment, as among the dasyurids.

but retain a rate of turnover of water and energy in hot or cold deserts intermediate between camel and cattle. The camel evolved in a North American desert and, in spite of migration through northern Asia, it still retains its desert functions and rates of metabolism. Water and energy handling rates may persist for millenia after migration to a different environment. Such functional diversity needs to be taken into account in introducing animals to new areas, or in making models of functions for a particular niche.

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Table 2 Water Turnover and Oxygen Consumption of Dasyurid Marsupials and Australian Rodents

		Water turnover/24 h		Pulmocutaneous evaporation/24h	Oxygen consumption/24 h		
		Weight	ml./kg	mol/l. ^{0.82}	ml./kg	l./kg	mol/kg ^{0.75}
Dasyurids							
<i>Sminthopsis crassicaudata</i>	(8)	17.4±2.3	460.8±68.4	17.2±1.9	234	112.9±13.0	1.91±0.24
<i>Dasyercus cristicauda</i>	(35)	86.9±22.1	133.9±36.0	6.7±1.2	95	36.1±6.0	0.87±0.16
<i>Dasyuroides byrnei</i>	(5)	130.0±18.0	124.9±22.1	7.2±1.1	58	29.8±5.8	0.81±0.19
Rodents							
<i>Notomys alexis</i>	(9)	34.9±3.7	192.4±56.0	9.3±4.3	86	74.4±21.3	1.26±0.24
<i>Notomys cervinus</i>	(11)	41.2±3.1	149.0±55.4	7.4±1.9	77	65.0±5.1	1.30±0.18
<i>Pseudomys australis</i>	(10)	49.9±12.1	139.0±25.9	6.3±1.3	89	62.7±13.9	1.32±0.25

Means and s.d.

The measurements were taken at temperatures near 25° C, with the animals at rest. Oxygen consumptions were averages over 1 h, and the water over 3–7 days.

The published determinations of metabolic rate⁸ in birds show that passerines have higher oxygen consumption rates than non-passerine birds, and that the water intake of passerines⁹ is correspondingly greater for the same size of bird than in non-passerines. Similar relationships seem to hold among plants in wet or dry habitats.

Other rate functions apparently linked to this system are the passage of labelled water from the rumen to blood, pulmocutaneous evaporation and the response of kidneys to vasopressin. Tritiated water moves rapidly from rumen to plasma in cattle so that plasma levels may reach as high as 90% of the equilibration value by 1 h, but in sheep only 50–60% of the final value is achieved in 1 h and the process is very slow in the camel with 25% in 1 h. These rates are presumably functions of the permeability of the rumen, but they follow the same rank order as water and oxygen metabolism. Evaporative transpiration of water through the skin and lungs ranks with water and energy turnover rates in small desert marsupials and rodents (Table 2) which do not have sweat glands. Similarly a standard reduction of diuresis or the increased excretion of potassium by ruminants infused with vasopressin requires fifty times more vasopressin in cattle than in the camel, while sheep or kongoni require three times the dose of camels, so that renal sensitivity to vasopressin is inversely proportional to water turnover. All these functions may be linked in the hypothalamus, but they also require coordinated cellular rate constants for handling water and energy.

Such disparate metabolic and physiological organizations in arid zone animals may arise from Pleistocene redistribution of genera¹⁰. Cattle radiated from Indochina where they seem to have evolved as wet tropical animals from ancestors of the guar and banteng. *Bos indicus* is less water dependent than other cattle, but all bovids whether in wet, cool, hot or dry environments appear to retain high metabolic and water turnovers. Sheep and goats evolved in dry mountains¹¹

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Table 3 Percentage Birth Rank Distribution by Sibship Size for the Total Patient and Control Samples

Sibship size		Birth rank										Not known	Total
		1	2	3	4	5	6	7	8	9	10		
2	Patients	55.4	44.6									0	372
	Controls	57.2	42.8									12	482
3	Patients	34.9	33.0	32.1								11	1,020
	Controls	32.9	34.5	32.6								8	527
4	Patients	25.3	26.8	23.3	24.6							9	913
	Controls	25.0	26.5	26.5	22.0							10	470
5	Patients	17.2	19.4	19.7	22.0	21.8						7	816
	Controls	16.6	15.4	20.9	24.0	23.1						10	360
6	Patients	16.0	15.7	15.5	17.1	16.4	19.3					8	691
	Controls	12.2	15.4	17.4	17.4	14.8	22.7					11	355
7	Patients	9.7	11.0	14.4	15.0	12.2	17.3	20.3				4	530
	Controls	12.4	9.9	15.7	16.1	13.9	16.4	15.7				5	279
8	Patients	8.5	9.8	9.8	9.8	13.7	11.4	17.8	19.8			8	395
	Controls	10.0	10.0	10.9	11.7	10.9	12.6	17.0				5	235
9	Patients	6.7	10.9	8.6	13.9	10.5	10.5	9.7	13.5	15.7		4	271
	Controls	11.7	12.3	6.8	12.3	11.7	7.4	13.6	11.1	13.0		6	168
10	Patients	5.1	8.3	6.9	11.5	9.7	10.1	11.1	7.4	17.1	12.9	2	219
	Controls	9.1	9.1	5.0	9.1	13.2	13.2	8.3	10.7	8.3	14.0	3	124

Table 4 Total Birth Rank and Reverse Birth Rank Distribution for the Total Patient and Control Samples

Birth rank	1	2	3	4	5	6	7	8	9	10	Not known	Total
Patients	1,425 (25.1%)	1,371 (24.2%)	952 (16.8%)	696 (12.3%)	454 (8.0%)	317 (5.6%)	226 (4.0%)	126 (2.2%)	79 (1.4%)	28 (0.5%)	53	5,727
Controls	742 (25.3%)	690 (23.5%)	509 (17.4%)	347 (11.8%)	230 (7.8%)	180 (6.1%)	114 (3.9%)	70 (2.4%)	31 (1.1%)	17 (0.6%)	70	3,000
$\chi^2 = 4.3649$, d.f. = 9, N.S.												
Reverse birth rank	Last	-1	-2	-3	-4	-5	-6	-7	-8	-9	Not known	Total
Patients	1,494 (26.3%)	1,550 (27.3%)	1,020 (18.0%)	676 (11.9%)	410 (7.2%)	263 (4.6%)	137 (2.4%)	77 (1.4%)	36 (0.6%)	11 (0.2%)	53	5,727
Controls	750 (25.6%)	817 (27.8%)	528 (18.0%)	320 (10.9%)	216 (7.3%)	130 (4.4%)	79 (2.7%)	49 (1.7%)	30 (1.0%)	11 (0.4%)	70	3,000
$\chi^2 = 10.7263$, d.f. = 9, N.S.												

Table 5 Percentage Birth Rank Distribution by Sibship Size of the Psychotic Patients compared with a Year of Birth Based Expected Distribution derived from the Control Sample (Table 1)

Sibship size		1	2	3	4	5	6	7	8	9	10	Not known	Total
2	Observed	58.4	41.6									0	89
	Expected	56.7	43.3										
3	Observed	40.7	28.3	31.0								4	117
	Expected	33.3	34.7	32.0									
4	Observed	22.0	26.0	28.0	24.0							1	101
	Expected	24.6	27.2	26.7	21.5								
5	Observed	16.0	15.0	17.0	20.0	32.0						0	100
	Expected	16.1	15.3	21.5	23.8	23.3							
6	Observed	15.4	14.1	7.7	25.6	17.9	19.2					0	78
	Expected	11.8	16.0	17.2	17.1	15.3	22.6						
7	Observed	15.2	3.0	13.6	9.1	18.2	18.2	22.7				0	66
	Expected	13.0	10.1	15.4	14.8	13.5	17.3	15.8					
8	Observed	6.4	8.5	10.6	4.3	14.9	4.3	21.3	29.8			1	48
	Expected	9.0	9.2	11.3	11.5	12.0	11.6	18.7	16.8				
9	Observed	7.9	10.5	2.6	13.2	21.1	7.9	15.8	7.9	13.2		1	39
	Expected	10.6	12.9	7.2	11.1	12.3	7.8	14.1	10.3	14.0			
10	Observed	10.7	3.6	7.1	3.6	10.7	7.1	7.1	10.7	28.6	10.7	0	28
	Expected	10.3	10.1	3.8	10.4	14.1	12.0	8.3	10.5	7.0	13.4		

$\frac{(O-E)^2}{E}$ sibs of 2, 0.1057, d.f. = 2, N.S.; sibs of 3, 3.2297, d.f. = 3, N.S.; sibs of 4, 0.6817, d.f. = 4, N.S.; sibs of 5, 4.8140, d.f. = 5, N.S.; sibs of 6, 9.1392, d.f. = 6, N.S.; sibs of 7, 8.1937, d.f. = 7, N.S.; sibs of 8, 9.9374, d.f. = 8, N.S.; sibs of 9, 4.3602, d.f. = 9, N.S.; sibs of 10, unreliable.

Table 6 Total Birth Rank and Reverse Birth Rank Distribution of the Psychotic Patients compared with a Year of Birth Based Expected Distribution derived from the Control Sample (Table 1)

Birth rank	1	2	3	4	5	6	7	8	9	10	Not known	Total
Total observed	167 25.3%	132 20.0%	103 15.6%	78 11.8%	76 11.5%	34 5.2%	33 5.0%	20 3.0%	13 2.0%	3 0.5%	7	666
Total expected	157.7 23.9%	151.4 23.0%	117.0 17.8%	81.0 12.3%	58.5 8.9%	40.8 6.2%	26.9 4.1%	14.8 2.2%	7.3 1.1%	3.8 0.6%		
$\chi^2 = 19.0184$, d.f. = 10. $P < 0.05$												
Reverse birth rank	Last	-1	-2	-3	-4	-5	-6	-7	-8	-9	Not known	Total
Total observed	180 27.3%	179 27.2%	132 28.0%	61 9.3%	48 7.3%	27 4.1%	16 2.4%	9 1.4%	4 0.6%	3 0.5%	7	666
Total expected	164.5 25.0%	178.0 27.0%	122.4 18.6%	73.9 11.2%	52.2 7.9%	29.4 4.5%	18.5 2.8%	10.2 1.5%	6.9 1.0%	2.9 0.4%		
$\chi^2 = 6.7060$, d.f. = 10. N.S.												

study of Hare and Price⁵ is far from conclusive for, as Barry and Barry⁹ have correctly pointed out, a sample of neurotics, or of any other non-schizophrenic patients, may well differ from the general population by birth rank distribution.

It is now clear that the estimation of expected birth rank distributions by the Greenwood-Yule¹⁰ procedure, which involves assuming an even distribution within each size of sibship, must be replaced by the direct comparison of psychiatric patients and an appropriate general population sample. The findings of such a comparative study from the north-east of Scotland are presented here. Tables 1 and 2 clearly show that, in the general population sample, there was a significant relationship between year of birth and both birth rank and reverse birth rank. Tables 3 and 4 present a detailed comparison of the birth rank distribution, for sibships of two to ten, of psychiatric patients and controls of similar year of birth distribution. In both samples there was an excess of first borns in sibships of two and of later born siblings in the larger sibships. The total birth rank and reverse birth rank distributions were strikingly similar in the two samples. The psychotic patients presented in Tables 5 and 6 were predominantly schizophrenic (ICD code numbers 300, 301.0, 301.2 and 303). They did not correspond by year of birth distribution with the control sample. Their birth rank distribution was therefore compared with a year of birth based expected distribution derived from the control sample. An excess of first borns in sibships of two and of later born siblings in the larger sibships was both observed and expected. The observed did not significantly exceed the expected in the later birth ranks of sibships of two to four. In the penultimate birth rank the observed and expected differed by only 0.2%.

It must be concluded that the positive findings of previous studies were due entirely to failure to take into account the very considerable effect of year of birth on birth order distribution and failure to compare patient samples with general population controls.

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A Non-parametric Alternative to d'

THE signal-detection theory propounded by Tanner and Swets¹ has proved immensely valuable and illuminating in a wide range of psychological contexts². It first gained popularity because of two signal merits: it accounted for the previously unexplained forms of the receiver operating characteristic (ROC) curve, and it enabled a numerical measure (d') to be put to the discriminability of stimuli.

The theory, however, makes a number of assumptions which are not necessary and may not always be true. Specifically, it is supposed that stimuli are represented internally by populations of signals which are both Gaussian in form and equal in variance. The general form of the ROC curve, however, is also predictable if other distributions are assumed—indeed, any which have a central “bulge” and longish tails. It is also not strictly necessary to assume equality of variance and there is some reason³ to suppose that variances are not equal. Unfortunately, it has not so far proved possible to distinguish experimentally between models which make the strict assumptions and those which do not. Further, attempts to examine subjects' responses in detail⁴ indicate that there may be considerable individual departures from the requirements of the model.

In practice, psychologists generally wish to ask two questions in a signal detection situation. These are: first, what is the relation between omissions and false positives—that is, what is the shape of the ROC curve? Second, how discriminable are the stimuli, or, more generally, how does the ease of distinguishing A from B compare with that of distinguishing A from C? The first question is generally, and satisfactorily, answered by asking for subjects' confidence ratings and so plotting the ROC curve directly. It is now usual to use d' from the second question.

Unfortunately, the use of this measure may, as we have seen, imply many assumptions which are simply not known to be true. The assertion (say) that d' for A and B is 1.61 and for A and C is 2.03 has a somewhat deceptive innocence. We are not merely asserting that it is easier to distinguish C than B from A. We are asserting that these quantities are represented to the subject internally by Gaussian distributions of percepts, whose variances are all equal and, moreover, whose form is perfectly known by the subject. This is rather more than is warranted by

our current knowledge. There seems, then, to be a need for a measure of discriminability which does not imply commitment to a particular model. Such a non-parametric measure should have the properties of being quick and simple to use, and should be monotonically related to d' , so that the rank ordering of results may be unaffected whichever measure is used.

For recognition experiments, Brown⁵ proposed just such an index. Pleasing though it was, however, it had originally the limitation that a number of examples of "noise" and of "signal and noise" had to be presented simultaneously. It is often desirable, and sometimes unavoidable, to present items sequentially. Brown has therefore modified his index (personal communication, 1971) for this condition. It is, however, computationally a trifle laborious, and there is clearly a need for a measure which is consistent and which can be derived as simply as possible from the data for the ROC curve. The following alternative is therefore suggested.

Let subjects be presented with a sequence of samples of As and Bs. They are asked to give confidence ratings from 1=certainly A to r =certainly B. Compute the mean rating for As ($=N$, say) and for Bs ($=n$, say). For perfect discrimination, $N=1$, $n=r$; for random responses $N=n$, clearly. Let C be the index, defined as

$$C = \frac{n - N}{r - 1}$$

It can be shown that C is monotonically related to d' , and it is obvious that $C=1$ for perfect discriminability, 0 for random response, and lies between 0 and -1 for consistent error.

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acting on a yielding cell wall". Many authors prior to them have written the same for 40 years, usually without the cautious "believed to be".

The conditions conducive to expansion are expressed in the following universally accepted way: $\psi_c = P - \pi$. . . , in which ψ_c means the water potential of the cell, P , turgor pressure, π , osmotic potential, and other terms have been left out in this context⁶.

If the tissue is in equilibrium with the ambient medium $\psi_m = \psi_c$, where ψ_m denotes the water potential of the medium. Actually the water permeability is usually so high that these two are always equal. That is why the first quoted phrase of *Nature's* correspondent is ambiguous. The origin of P is, as mentioned, the resistance of the cell wall to expansion. A cell ceases to take up water when $\psi_m = \psi_c$ and P reaches a maximum. Turgor is built up because there is an expansion. Thus turgor does not cause expansion and is not the driving force. What this is I shall show later.

First I shall consider growth. It begins with a loosening of the wall, demonstrated by Cleland and many others. It can certainly take place in different ways⁷. It implies that the resistance to an expansion decreases, P would attain a value P_x with $P > P_x$. Thus $\psi_m > \psi_c = P_x - \pi$ This causes an immediate expansion. The driving force is the difference $\psi_m - \psi_c$. After expansion $\psi_m = \psi_c = P' - \pi'$. . . , with P' and π' lower than P and π . This is elementary and was formulated in 1920 with other symbols. The driving force of any expansion is a difference in water potentials. Expansion is due to water uptake. Volume changes during growth should best be expressed in terms of water fluxes⁸. Unfortunately, the phrase "turgor expands the cell" was coined in 1931 and has been copied dogmatically in untold articles, although the mistake has been pointed out repeatedly^{9,10}.

What the erroneous opening "turgor expands the cell" may lead to is shown by Green *et al.*⁵ who assume as an axiom or a corollary that growth (r) is proportional to P : $r = mP$. Since experiment fails to submit to the assumption, they introduce a correction factor, Y , called yielding threshold: $r = m(P - Y)$. This does not suffice because growth is practically independent of turgor. Thus they assume that Y shifts with P , otherwise $P - Y$ will not remain constant. It seems to me that little is left of the proportionality between P and growth in this instance.

The mistake made since 1931 is to regard the cell as something existing in an empty space, disregarding the ambient medium and external hydraulic pressures⁵.

The literature on plant cell growth would certainly improve if the notion of turgor expanding the cell was abandoned and replaced by accepted equations for water balance or fluxes. They must be valid in growing as well as non-growing cells, perhaps with the addition of terms denoting cell wall changes¹¹ in growing cells.

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Wishful Thinking of Turgor

REVIEWING work by Rayle *et al.*¹ a correspondent has written² that plant cell growth is a simple process, and continues, "There is a strong osmotic tendency for water to enter a . . . growing cell, but this is prevented by the rigidity of the cellulose walls". (Hydroxyproline bridges in the wall limiting growth are located in the amorphous matrix; hence a unique role of cellulose as resistance in the wall is questionable³.) The first sentence is dubious. Cleland⁴, however, has expounded the matter: "This potential for extension can . . . be converted by turgor pressure into . . . wall extension". This adds to the doubts because the rigidity of the walls preventing the entry of water is the cause of the turgor pressure, which arises as a consequence of the resistance of the wall to an expansion. How then can the turgor pressure cause expansion? A simple answer is given by Green *et al.*⁵: "Plant cell growth is . . . believed to be the result of a driving force, turgor pressure,

BOOK REVIEWS

Kammerer *Redivivus*

The Case of the Midwife Toad: The Paul Kammerer Controversy. By Arthur Koestler. Pp. 187+10 photographs. (Hutchinson: London, September 1971.) £2.

OF all the gadflies that have harassed the community of science in recent years, none has been more stimulating than Arthur Koestler. He has always been at his best when taking learned snipes at scientific orthodoxies. In *The Case of the Midwife Toad*, he follows true to form with a uniquely constructed attack on neo-Darwinism.

The vehicle for his argument is the tragic case of Dr Paul Kammerer (1880–1926). Kammerer was a Viennese zoologist, a minor composer, an amorous lover and a committed socialist. As a biologist, he combined considerable experimental skills with a neo-Lamarckian outlook, to produce a series of investigations on sea-squirts, salamanders, lizards and toads which demonstrated to his satisfaction that physical changes effected by an organism's adaptation to a given environment are "preserved" by heredity and genetically transmitted to its offspring. For Kammerer the crucial experiment involved the sea-squirt (*Ciona intestinalis*), but his neo-Darwinian opponents—based in England and led by William Bateson—concentrated their attention on his results concerning the midwife toad (*Alytes obstetricans*). Ultimately this dispute boiled down to a challenge of Kammerer's integrity as an investigator. When, in the summer of 1926, it was shown that his sole remaining specimen of midwife toad was (or had been rendered) a fake, his antagonists appeared to be vindicated. Shortly afterwards Kammerer committed suicide, an action which effectively discouraged further interest in his work.

Why then has Koestler seen fit to return to this distant controversy? His answer is straightforward: "The secret of (Kammerer's) attraction lies partly in his complex character and tragic fate, but above all in his heretical ideas, that is, Lamarckianism". Since this is a heresy for which Koestler has expressed a guarded sympathy in the past, we might expect him to exhibit some tell-tale signs of bias.

On the central issue of Kammerer's honesty, however, the author is patently sincere when he argues that "I did not start with the intention to rehabilitate Paul Kammerer, but I ended up with an attempt to do so". Moreover, I believe that Koestler has achieved his imme-

diate purpose. By virtue of diligent reading and of interviews with survivors of the controversy, and an ingenious simulation of the original forgery, he has managed not only to establish his subject's innocence but to provide as well good reasons for performing Kammerer's experiments again. Here Koestler should earn our profoundest admiration.

Unfortunately, the combination of his initial triumph and philosophical presuppositions has placed too great a strain on whatever empathy the author may ever have possessed for the early neo-Darwinians. Bateson and his cohorts are thus depicted as "the defenders of the new orthodoxy" and, even worse, "an inhuman Establishment" whose campaign of defamation was the essential precondition for Kammerer's death. Faced with this description, I would maintain that Koestler has viewed his material through the wrong end of the right microscope.

For example, he is correct to stress the importance of the participants' attachment to dogma but his portrayal of this tendency as "unscientific" reflects an inadequate understanding of the social processes of science. Drawing on the work of Kuhn and others, it is surely evident by now that fidelity to a conceptual paradigm is a necessary concomitant of "normal" scientific research. When a new school of thought attempts to displace an older one, the resulting boundary disputes are bound to be bitter and protracted. For the stakes are not merely intellectual, but also institutional: to the victor will go the spoils of future students and funds for research.

It is in this light that we must examine the reception of Kammerer's work in England. At that time it was classical zoology, not experimental genetics, which constituted the "establishment". Bateson's bastion was indeed a beleaguered one, defending itself along two fronts against statisticians and zoologists. By the 1920s, the generals and lieutenants of the respective armies were battling regularly over such issues as religion, reductionism and eugenics, not to mention neo-Lamarckianism. Whenever a new result seemed to support one side or the other, it was certain to be brandished eagerly and attacked vigorously (most often in the columns of *Nature*). This was the fate meted out to Kammerer, once he was placed in the centre of such an academic maelstrom. Ironically, his defeat did not immediately further the instrumental aims of the geneticists. As late as 1935, it was

still possible to count the number of UK professorships and studentships in genetics on one hand; the numerical pre-eminence of the zoologists was still undiminished.

Lamarckianism, on the other hand, was virtually eliminated from the picture (in Britain). To the lay reader of Koestler's book, this might seem startling, because the author has failed to give a fair comparison of the relative strengths of the two sides in the 1920s. Placed against the achievements of the schools of chromosomal heredity and population genetics, the neo-Lamarckians could only muster a set of highly disputed experimental results. Even on the assumption that their data were correct, they had no plausible (physiological) theory to explain how environmental adaptations could alter the hereditary material. Apparently Kammerer was no exception to this rule.

Thus did a new orthodoxy arise to be transformed triumphantly in a few decades into the new field of molecular biology. Koestler does concede (without sufficient emphasis) that the orthodox have often differed among themselves as to the conclusiveness of the neo-Darwinian synthesis. English left-wing scientists, in particular, have often hedged their bets on this one.

The mention of politics reminds us that theories of heredity necessarily have ideological significance: what kind depends on the socio-historical context in which they are worked out. In nineteenth-century Britain liberals tended to be hereditarians and conservatives talked like environmentalists. About seventy years ago these linkages were reversed. This helps to explain why Kammerer's chief English defender, the elderly zoologist E. W. MacBride, was as far to the right as his Austrian colleague was to the left. As a prominent Bolshevik sympathizer (indeed, because he was one), Kammerer was invited to establish an institute in the Soviet Union. But so too was the geneticist H. J. Muller. Contrary to Koestler, the Russian scientific establishment was not at that time "committed by the party line to the Lamarckian theory of evolution". When Lysenko's school did finally emerge, few Western biologists were prepared to face the political and professional sanctions of espousing any form, no matter how diluted, of neo-Lamarckianism.

Now, in the eighth decade of this century, environmentalism is reasserting itself within the life sciences. Witness the growth of animal behaviour, developmental biology and ecology. Even

at the molecular end of the spectrum the faith of an earlier time in Crick's "central dogma" is not quite secure. When these factors are added to a resurgence of organicist philosophy and a revulsion against Jensenism, Koestler expects us to wonder whether neo-Darwinism will be modified by any Lamarckian additions. Koestler envisages just such a possibility. And so we shall

PAUL GARY WERSKEY

Newton as Geometer

The Mathematical Papers of Isaac Newton. Edited by D. T. Whiteside. Vol. 4: 1674–1684. Pp. xxxii+678+4 plates. (Cambridge University: London, June 1971.) £18.

EACH new volume of Whiteside's exemplary edition of Newton's mathematical papers adds significant features to the picture we may try to form of this secretive genius. The present volume reveals Newton as a geometer: a third of it (part II) is devoted to remarkable geometrical research, datable from the years 1678–80 and in large part not previously known; another third (part III) reproduces unfinished drafts of treatises on fluxional analysis (about 1680) and computation by series (1684), which cast more light on the essentially geometrical character of the novel conceptions and methods he used in the *Principia*, but only grudgingly and inadequately published as a lemma in the second book of that work. Also in the remaining part of the volume, in which papers of more algebraical or arithmetical content from the decennium 1674–84 are collected, Newton's geometrical turn of mind is apparent, not only in his trigonometrical treatises (of mainly didactic interest), but even in his treatment of Diophantine problems.

Once more the documents (abundantly annotated by the editor with unfailing erudition and shrewd judgment) do justice to silly tales endlessly repeated by scientists lacking historical sense. This time, they expose the inanity of speculations too often expressed about the geometrical style of the *Principia*: how likely indeed that Newton would have deliberately clothed in ancient geometrical garb results of purely analytical computations! As Whiteside rightly points out, Newton's style was thoroughly in the modern tradition initiated in the sixteenth century and given its characteristic expression by the Cartesian school: this tradition built, of course, on the conceptions of the ancient geometers, but it blended them with the algebraic methods which had hitherto been left to develop along their own lines in the hands of practical men. In matters of mathematical thought and style, the innovator was Leibniz, not Newton. The fluxion idea was the most strikingly successful pro-

duct of this blend of geometry and algebra: the kinematical conception of a curve as a trajectory resulting from the composition of two motions had been revived by the Italians and brought to Cambridge by Barrow; Newton's decisive step was to algebraize it—a step nearly missed by René de Sluse, who was familiar with Torricelli's kinematical method of drawing tangents to curves and also knew in special cases how to derive an algebraical expression for the subtangent from the Cartesian equation of the curve. Newton's success is due to his superior grasp of the algebraical side of the problem, embodied in his method of series expansion. The third part of the volume under review contains in this connexion documents of the highest importance, in which we see Newton endeavouring (not altogether correctly) to answer Leibniz's queries about the universality and completeness of his method. Had these drafts not been put aside when Newton concentrated on the writing of the *Principia*, the later priority squabble might have been avoided, or at least might have taken the more useful course of an objective confrontation of the two rival approaches.

The most surprising items, however, are found in the second part of the volume, among Newton's unpublished papers on geometrical topics. They give us the clue to Newton's motivation for including in the first book of the *Principia* the purely geometrical section V on the construction of conics. It appears that he wanted in the first place to challenge Descartes's boastful assertion that his analytical solution of Pappus's problem transcended the scope of the ancient methods. Indeed, as we now learn, Newton produced a general solution based on simple geometrical reasoning, which he regarded as a tentative restoration of the hidden analyses performed by the ancient geometers. This led him, however, rather naturally to a more general and deeper investigation of the universal characterization of conics: the tract on this subject, reproduced in the volume, and of which only the beginning was inserted in the *Principia*, is a masterpiece which places Newton among the very few geometers of the time who rank as precursors of projective geometry; it shows in fact that he not only recognized the fundamental projective properties of the conics, but explicitly introduced the notion of projective correspondence. The material here made available (the editor promises us more in the same vein for later volumes) allows us to understand the true meaning of Newton's admiration for the ancients: he was better than anyone entitled to appreciate their methods after he had rejuvenated them and given such proof of their latent power. L. ROSENFELD

Power and Environment

Power Generation and Environmental Change. Edited by David A. Berkowitz and Arthur M. Squires. Pp. xxiii+440. (MIT: Cambridge, Massachusetts, and London, 1971.) \$16.95.

THIS book is the proceedings of the symposium on "Power Generation and Environmental Change" held by the American Association for the Advancement of Science at the annual meeting on December 28, 1969.

The book is divided into five main sections, beginning with a short discussion on the broad aspects of the main problem: the impact on the environment of the steadily increasing demand for energy coupled with an increasing population. What effects will this have on the quality of our air and water and on the ecology of plants and animals? Two approaches are possible to control harmful effects: the first is to use even more technology to prevent air and water pollution and to recognize that society must bear the cost; and the second is to ration the consumption of energy per head and to limit the increase in population.

The second section deals with nuclear power stations and possible effects of radioactive releases on public health and safety. It discusses only light water reactors, of the boiling-water or pressurized-water types, and gives some data on the radionuclides released in the liquid effluent and gaseous emission. As far as gaseous emissions are concerned the pressurized water reactor in normal operation produces even less than a coal-fired power station, which it is agreed presents a negligible hazard. The emission from a boiling water reactor is, however, at least four orders of magnitude greater. Several contributions discuss the relation between radiation dose and cancer, the possible genetic consequences, and the radiation dose limits which should be adopted to reduce such risks below "natural" or other more familiar risks. It is here that there is the greatest controversy, and estimates by different authors of a radiation exposure situation vary by a factor of up to 10,000.

Hydroelectric generation and pumped storage systems, discussed in the third section, need have no adverse overall effect on the environment, and in fact often make it possible to provide improved facilities to enjoy the countryside. Nevertheless the author of the contribution on ecological effects paints a frightening picture of "a sepulchral forest of submerged trees", and "a man whose home is drowned, whose form of livelihood is interrupted, whose urine turns to blood, whose eyes go blind, or who shakes with malaria because a lake was created".

The fourth section is devoted to power

generation from fossil fuels, coal and oil. The main environmental problem is to reduce the effects of sulphur dioxide, either by dispersion using tall stacks, or by limiting the emission by removing the sulphur from the flue gases at the power station, or from the fuel oil at the refinery. The climatic consequences of the accumulation of carbon dioxide in the atmosphere are discussed, and there are some interesting contributions in the new field of atmospheric chemistry—the study of “photochemical smog”, and the life cycles of sulphur dioxide and nitrogen oxides on a continental or a global scale.

The final section on “waste” heat is disappointing. There is far too much repetition; the same basic information is presented many times with variations which must confuse the non-specialist reader. For example, “nuclear plants require 40 per cent more cooling water than fossil-fuel plants” (p. 345); “nuclear plants waste 60 per cent more energy than fossil-fueled plants” (p. 353); and “fossil-fuel plants discharge approximately one-third less waste heat to the cooling water than do nuclear plants” (p. 369). In only one instance is it pointed out that future designs of nuclear plants will not require more cooling water than fossil-fuel plants. Two of the authors use the term “thermal pollution” for warm water discharges and emphasize the possible deleterious effects and the need for the control of temperature rises to a few degrees (F). The other contributors to this section take a more constructive approach and point out that the problem can be solved by the proper siting of power stations and the design of cooling systems, and that beneficial uses of “thermally enriched” water can be developed.

Summing up, the book is rather a mixed bag, but the patient reader can assess the various contributions for himself and obtain an overall picture of the American environmental scene—which, of course, is not necessarily relevant in Britain.

J. S. FORREST

Invertebrate Fossils

Atlas de Paléontologie des Invertébrés. By René Verniory. Pp. 217+89 plates. (George Librairie: Genève; Masson et Cie: Paris, 1970.) 190 francs.

THIS volume has been published after the author's death. The wholly laudable desire to ensure that the dead man's work has not been in vain cannot gloss over the real difficulties involved. Most of the deficiencies of the book can be ascribed to this.

The plan of the book is one which I have met before in works published in Switzerland. It consists largely of a systematic summary of the salient facts about the palaeontology of the in-

vertebrate phyla, these being illustrated by sketch diagrams on fold-out sheets. These fold-out illustrations make up about half the thickness of the volume.

The criticisms of the book—understandable under the circumstances of publication—are: failure to cover some invertebrate groups at all; a sometimes obsolete classification—it has been known since the work of Kozłowski in 1938 that the graptolites are not coelenterates—and an utterly inadequate bibliography. For these reasons the work cannot be considered as a standard work of reference and its price would, I think, effectively prevent its sale in England for any other reason. It is clear why the book is so expensive—the cost of the many fold-out pages must be high. This does not alter the hard fact that only an indispensable reference work—the *Traité de Paléontologie* itself is an example—can hope to have a sale when its price is more than £20.

K. A. KERMAK

Cancer of the Lung

Morphology of Experimental Respiratory Carcinogenesis. Edited by P. Nettesheim, M. G. Hanna, jun., and J. W. Deatherage, jun. (Proceedings of a Biology Division, Oak Ridge National Laboratory, Conference held in Gatlinburg, Tennessee, May 13–16, 1970.) Pp. xiv+483. (National Technical Information Service, US Department of Commerce: Springfield, Virginia, 1970.) \$6.

THIS book covers the proceedings of a conference which was a logical sequel to one held in 1969 and reported in the *AEC Symposium Series* No. 18. The earlier meeting concentrated on methodology whereas the papers given at the 1970 symposium, although they still include a considerable amount of experimental detail, lay the main emphasis on the resultant lung changes as seen by light and electron microscopy. This has been supplemented in some cases by histochemical studies.

The experimental species used by different authors vary from the usual laboratory rodents to the white Pekin duck, thus there must be considerable variation of normal lung structural detail. Nevertheless a useful if fairly generalized synopsis of the main cell types found in respiratory tissue is given with some histochemistry. The study of cell kinetics is limited to rats and mice.

Although the bulk of the material concerns experimental oncogenesis, there are also papers on human occupational hazards which may lead to cancer of the respiratory tract. Particular reference is made to the uranium mining and asbestos industries and the influence cigarette smoking may have on individuals exposed to these hazards. Much evidence supports the

idea that the induction of respiratory cancer can be multifactorial and such a possibility has not escaped the notice of the scientists, a number of whose experiments are designed to examine this facet of the problem.

Many of the investigations are to establish biological models for the future evaluation of changing (and it is hoped, improved) environments either natural or industrial; others are attempts to elucidate the mechanisms by which a particular carcinogen acts. It is important therefore that there should be uniformity of nomenclatures and a description of the WHO classifications of human lung tumours is given. In the later “Panel Discussions” some tentative ideas are put forward for similar but less detailed classifications of natural and experimental animal tumours. It is certain that the majority of experimentalists will welcome this move and the project is being continued by the International Agency for Cancer Research at Lyon.

This is a book essentially for those actively involved with the problem of experimental respiratory carcinogenesis to whom it is well recommended. Some of the work reported is not entirely new but many of the papers are by recognized world authorities in this field of research. These proceedings therefore make interesting and informative reading as well as being a useful source of reference material in this still rather disputatious area.

BRIAN R. DAVIS

Hydrogen Bonding

Hydrogen Bonding. By Serge N. Vinogradov and Robert H. Linnell. Pp. xi+319. (Van Nostrand Reinhold: New York and London, July 1971.) £4.75.

THE aim of this book is to review hydrogen bonding and the many and important applications of this phenomenon. The book is stated to be written particularly for undergraduates and post-graduate students and research workers in the life sciences. Because of the breadth of topics and wide range of experimental techniques that are used in studying hydrogen bonding, this is not an easy task to undertake.

In my opinion, the concept of the book is admirable and timely. In addition, with a few exceptions, the balance of topics chosen for the different chapters is also very good. However, the book falls down very seriously in execution and contains an unusually large number of errors. For example, the very first figure, Fig. 1-1, is wrong, Fig. 1-4 is poorly and inaccurately drawn, and Fig. 2-1 is also wrong. Such rather obvious mistakes which occur at the beginning of the book are also to be found not infrequently in later chapters. Another example is given by the first

paragraph in section 2-7 where the changes in molar volume that can be correlated with hydrogen bonding are discussed in a confused manner. This is so much so as to give the impression that the phenomena concerned are quite different in organic chemistry and in inorganic chemistry! Again, on page 50 it is stated that "The force constant k is . . . directly proportional to the frequency (ν) and indirectly proportional to the square root of the reduced mass . . .". This statement is in contradiction to equation (3-2) on the previous page. Also on page 50 the diagrams of the vibrations of a water molecule are crudely and partly incorrectly drawn. Because of errors of this type the book cannot be recommended for basic teaching purposes. The problems set at the end are rather repetitive in type, mostly involving equilibria deduced from spectroscopic measurements.

Nevertheless the balance and coverage of the literature are good and the book will undoubtedly be useful to many more expert scientists who will be pleased to see the salient aspects of the subject spelled out, and who are not likely to be put off by the errors scattered about. I have the impression that the book has been written in too much of a hurry and as a result is of much lower standard of execution than it ought to be. Nearly all the errors noted would almost certainly have been corrected if the authors had asked experts in the subject-matter of the various chapters to read and comment on them before printing. It could still be worthwhile to do this for a possible later impression.

N. SHEPPARD

Dyes and Pigments

Colour Chemistry. By R. L. M. Allen. Pp. xii+336. (Thomas Nelson and Sons: London, 1971.) £5.

FOR several years there has been a need for a modern introductory textbook on colour chemistry, and this work by R. L. M. Allen is the second book to appear in the last three years intended to fill this gap. The author has indicated that the book is presented as an introduction both for specialist colour chemists and for students whose main interests lie in related branches of pure chemistry or technology. Consequently a comprehensive coverage of the field has not been attempted, and the reader is frequently referred to the more specialist literature where a deeper treatment is required.

The development of the subject-matter of the book follows well established lines, and after introducing the reader to the physical basis of colour and the history and classification of dyes, subsequent chapters deal with classes of dyestuffs largely according to their chemical constitution. Pigments

are briefly dealt with, and to illustrate the less obvious applications of colour chemistry, chapters on fluorescent brighteners and colour photography are usefully included.

It is obviously very easy to criticize the content of a book of this type, since the selection of material is very much a matter of personal choice. Generally, however, the field has been well covered, and the more obvious omissions that have been made, for example the intermediates of dyes and pigments, do not detract from the value of the book. Perhaps a more serious omission is the absence of a discussion of the mechanistic principles of diazotization and coupling, two of the most important processes of colour chemistry. The theoretical treatment of colour and chemical constitution is brief but adequate for a book of this level, but surprisingly few practical examples of empirical colour relationships are given. The new student is given little indication of the position and type of auxochromes necessary to give, for example, red monazo dyes, or blue anthraquinone dyes.

The author has succeeded in presenting material relevant to modern colour chemistry, largely by drawing on his own industrial experience and by frequent reference to the recent patent literature. The latter is commendable in a field where disclosures of important technological developments often only occur many years after their introduction. One criticism concerns the brevity with which disperse dyes are treated by comparison with dyes for natural fibres, which does not reflect the true technological importance of the former dyes at the present time. A heavier emphasis on disperse dyes for synthetic fibres, perhaps at the expense of azoic and direct dyes, would have presented a more accurate picture.

This is a readable, well presented book which will provide the reader with a useful, up to date background to colour chemistry.

J. GRIFFITHS

Fish Adaptation

Explorations in the Life of Fishes. By N. B. Marshall. Pp. 204. (Harvard University: Cambridge, Massachusetts; Oxford University: London, October 1971.) £2.30.

A SERIES of lectures given at Harvard in 1963, and refurbished in 1964 and in 1967-8 for the Universities of British Columbia and of Miami, is at last published in revised form. The revision must have been thorough, for a large proportion of the bibliographic references are (as one would hope) subsequent to 1963.

Marshall's *The Life of Fishes* was published in 1965, during the gestation period of this book (and it may well be that publication of this book was

delayed for that reason). The two books are, however, very different. The earlier one was large, comprehensive in scope, and apparently intended to be accessible to the lay enthusiast as well as to professional zoologists. The new book is smaller, more selective and seems to assume substantial ichthyological knowledge in its readers, who are expected to be familiar with the wide variety of fishes cited as examples and to be enlightened by remarks such as "one has only to think of the gobies". There is an appendix which deals briefly with the classification of teleosts, but this will not be much help to the novice, particularly as it is unillustrated and includes no references to illustrations of fishes elsewhere in the text. Technical terms such as "hypural" and "stenobathic" are used without definition and the reader is expected to be able to fill in for himself the explanations behind statements such as "Yet a large swimbladder has disadvantages: for the greater its capacity, the more effort a fish must exert to keep station after a given rise or fall above a level of equilibrium".

The book has three main sections. Chapters 1 and 2 discuss the success of teleosts and some features of their design for swimming. Chapters 3 and 4 are about the specializations of deep sea teleosts and chapter 5 is a long discussion of convergent evolution. The discussion of success is extremely interesting, though some of the main arguments are only sketched out. The section on deep-sea teleosts is especially welcome on account of Marshall's unrivalled knowledge in this field. The section on convergent evolution overlaps rather awkwardly the section on deep-sea fishes; the eyes, lateral lines and swimbladders of deep-sea fishes are discussed in both sections and there is even a part of the convergent evolution chapter specifically about convergences of deep-sea fishes. I would have found this chapter more interesting if fewer examples of convergences had been considered and the space so saved used to show in a few cases precisely why it is believed that certain resemblances are due to convergence rather than to common ancestry.

The book is illustrated by line drawings which are apt, informative and attractive, but are sometimes rather too small for comfortable study. This is because the text has been set up so as to fill only two-thirds of the width of each page, and most of the illustrations have been squeezed into the margins.

This is a stimulating book and I am glad to have my copy, but it would be more generally useful if it had been written with more thought for the reader who may be a sound zoologist but is not particularly knowledgeable about fishes.

R. McNEILL ALEXANDER

CORRESPONDENCE

Insecticides

SIR,—Worthington (*Nature*, 234, 55; 1971) states positively that "the proposed banning of DDT in the USA will stimulate the search for better insecticides and integrated pest control . . ." and the "efforts in these directions remain at a relatively low ebb so long as the bludgeon of DDT and other persistent synthetics is readily available". His argument was somewhat damaged by your footnote. Also, his premise is the reverse of the obvious facts; the success of DDT was a great boost to the search for other and better insecticides, just as the success of the Ford car triggered competition in the automobile industry. Many millions of dollars were spent in insecticide research in the past two decades and the stimulus for the expenditure was reinforced by the onset of resistant strains. The WHO has tested more than 1,400 new insecticidal compounds since 1960 for suitability in vector control of malaria.

The use of pesticides has furnished bread, and now the attack on them provides circuses. Contrary to Worthington's reasoning, several large organizations that searched for new insecticides are weary of being pursued by environmental lions ("a public relations headache"), and are leaving the Colosseum (*Chem. Eng. News*, 16, 1971). For example, Standard Oil of New Jersey has put its pesticides research programme up for grabs. If new insecticides are needed for integrated control, how long would it take to "clear" one—five years? Almost any substance will produce tumours, or deformations in embryos, if enough is injected, and there is always the question of "long-term effects" which are too subtle to detect, but not too subtle to be used in arousing fear of the unknown.

Yours faithfully,

THOMAS H. JUKES

University of California,
Berkeley

Arms Control

SIR,—Ralph Lapp ends his review of the "Impact of New Technologies on the Arms Race" (*Nature*, 234, 273; 1971): "The United States appears to be racing itself, not competing with the Soviet Union, as it seeks to exploit military technological opportunities."

He seems to be implying that the United States *alone* is arms racing (rather than that the form of US arms racing does not relate to the form of

Russian arms racing). The present situation surely is that while both superpowers continue to proliferate nuclear weapons, the Soviet Union has already substantially overtaken the United States in numbers of ICBMs, in megatonnage, and in underground tests of large nuclear devices; that the Soviet Union is approaching parity with the United States in missile launching submarines, has already flown a new supersonic long range bomber, has long since had an ABM system deployed and an Orbital Bombardment system, and now has a non-nuclear anti-satellite capability as well.

As one who believes, passionately, that arms control and disarmament are a necessary (though not sufficient) condition of human survival, my heart sinks every time an American dove proclaims his belief that today the United States alone is arms racing or exporting weapons or whatever. This was indeed virtually so when many of today's doves were working with the Kennedy administration, but it has not been the case for the last half decade or more: is it not time they recognized that today the Soviet Union is acquiring and proliferating weapons of all sorts at least as vigorously as the United States then did?

Yours faithfully,

ELIZABETH YOUNG

100 Bayswater Road,
London W2

Ocean Conservation

SIR,—The terrestrial environments of the Earth have been divided by ecologists into a series of "biomes", according to variations in climatic and geophysical factors. Within these biomes, a large variety of different types of habitat have been recognized, each with more or less distinct floral and faunal characteristics.

The oceans of the world, which cover 70.8% of the Earth's surface and contain 80% of known living organisms, can also be divided into distinct regions, according to climate and depth. These regions have distinct faunal and floral characteristics, as do the terrestrial biomes, although the distinction is considerably blurred by the activity of the ocean currents and mixing of separate water bodies.

Nearly all nations now have devoted thought and effort to conservation of their natural resources by the establishment of parks, reserves, sanctuaries

and so on, setting aside areas for research, education and recreation. A question now occurs to me. What steps are being taken to do the same for the marine habitats?

The Annual Report of the President to the Congress of the United States of America on Marine Resources and Engineering Development in April 1970 gives a brief but good summary of marine pollution. But throughout the report, the ocean is referred to in the singular. Further on, the same report mentions that many countries are taking steps to set up conservation areas in their coastal waters. Coastal environments are distinguished as estuaries, lagoons, beaches, wetlands and so on. Some countries have established underwater marine parks inside their territorial waters. Should not the open oceans be recognized as a series of environments, each worthy of some attempt at conservation?

With the fast developing technology of marine mining and underwater engineering, the declaration of protected areas in the ocean should not be left too late. Benthic faunas in particular would suffer in large scale dredging and boring operations. So far, open ocean conservation has consisted of the protection of single species from overexploitation. Terrestrial conservationists have found out that preservation and management of environment is a better approach.

There are tremendous problems associated with the setting-up of open ocean protected areas, different from those faced by terrestrial conservationists. A few spring to mind. (1) Which areas to choose? This is relatively simple, and can be solved by present surveying methods, if enough time is available. (2) Who would control the proposed areas? Because they would in most cases be outside territorial limits, the responsibility would fall to the UN. Costs should be shared by the member nations of the UN. (3) Management problems, such as counteracting pollution by contamination from outside, would arise. These are some problems, but it must be agreed that attempts must be made to set up these areas, and research into management methods must be worthy of attention.

Yours faithfully,

JAMES MILLER

University of San Carlos,
Cebu City,
Philippines

Obituary

Dr Jean Nitsch

ON June 28, 1971, Dr Jean P. Nitsch died on the Brittany coast while diving to study marine plants. It was typical that his zest for observation and inquiry should lead him into new and unfamiliar techniques, and his death, at 49, has robbed France and plant physiology of a very versatile investigator.

Born on September 15, 1921, Nitsch's career was divided between his native France and the USA where he was equally at home and as well known. As in the case of most of his generation the Second World War interrupted his Baccalaureate studies, but in due course he acquired diplomas in France from the University of Grenoble (1943), the Sorbonne (1946) and as Ingénieur Agronome de l'Institut National Agronomique de Paris (1947). But it was his association with Professor F. W. Went at the California Institute of Technology, Pasadena, which culminated in a doctorate awarded in 1950, that first brought him to the attention of the scientific world. The title of his thesis, *The Role of Plant Hormones in Fruit Development*, indicates the trend of Nitsch's work throughout. He was dedicated to the interpretation of the controls of growth through chemical regulation and strongly motivated to work in areas beneficial to horticulture.

Even before the presentation of his thesis Nitsch's work became known because of his attempts to culture immature fruits aseptically, *in vitro*, and early papers appeared both in France and in the USA¹⁻³. In this early work a favourite experimental material was the multiple strawberry fruit, and Nitsch drew attention to the role of achenes in the development of the receptacle and of auxin (IAA) in the growth and morphogenesis of the strawberry. From this starting point Nitsch moved rapidly to the culture of excised ovaries, to the general use of proliferated callus cultures in the study of growth, and to a variety of investigations concerned with the role of hormones in sex expression and the

development of fruits^{4,5}. His interest in these fruits was maintained at Harvard (1950-51) as a postdoctoral research fellow and, after a brief return to France, at Harvard again as a research fellow (1953-55) where he worked in the laboratory of, and was influenced by, Professor R. H. Wetmore.

From 1955 to 1958 Dr Nitsch was an assistant professor at Cornell University and was in charge of a course on plant physiology applied to horticulture. His role in the Department of Floriculture at Cornell allowed him to exploit his interest in the use of aseptic culture procedures, while he also interested himself in the problems of plant propagation, in photoperiodism, and in the chemical induction and regulation of growth.

After returning permanently to France in 1958 Nitsch was associated (in obvious continuity with his experiences at Pasadena) with the development and use of the Phytotron, a large installation for the environmental control of the growth of plants, at Gif-sur-Yvette near Paris, and he served as deputy director there from 1958-62. It is in this capacity and successively as Directeur-Adjoint d'Institut de Recherche au CNRS (1962-69) and as directeur du Laboratoire de Physiologie Pluricellulaire from 1969 until his death that Nitsch's scientific stature came to be recognized internationally. Some 35 investigators from 16 countries were attracted to the Phytotron during the years of his association with it. Nitsch was also appointed to an academic position in France as professeur à l'Institut National Agronomique in a chair designated for plant physiology.

In conclusion, certain areas of Nitsch's wide range of interest may be mentioned. The early work on auxin and growth promoting substances prompted him to show by chromatographic means that many naturally occurring substances had auxin-like activity. This interest was to make him a welcome participant and contributor to a succession of international conferences on plant growth regulating

substances. Nitsch contributed to the conference held at Wye, Kent, in 1955⁶, and the maintained interest in chemical regulation of plant growth led him to organize a similar international conference at Gif, the proceedings of which were published in 1964 under his editorial supervision⁷. Any full current treatment of the chemical controls of growth must note Nitsch's work on cell division or cell enlargement, using a variety of assay systems and as brought about by a variety of substances. His work has involved auxins, cytokinins, gibberellins as well as substances and extracts held to be more specifically associated with flowering^{8,9}.

In logical continuation of Nitsch's interests in aseptic culture of somatic tissues and floral organs, he intervened most effectively in the culture of haploid cells from anthers of tobacco¹⁰ and studied their development into haploid plants. By subsequently doubling their chromosome numbers, Nitsch obtained, in this way, completely homozygous plants from certain mutant forms and published in this field earlier this year¹¹. Nitsch's published work includes at least 154 papers and a number that will appear posthumously.

No reference to Jean Nitsch would be complete, however, without tribute to his friendliness and the hospitality that he and his wife, Colette, extended to so many scientific visitors to France, his laboratory, and his home in Gif. He is survived by his wife, Dr Colette Nitsch, and by two children.

¹ *CR Acad. Sci., Paris*, **229**, 445 (1949).

² *Science*, **110**, 499 (1949).

³ *Amer. J. Bot.*, **37**, 211 (1950).

⁴ *Quarterly Review of Biology*, **27**, 33 (1952).

⁵ *Ann. Rev. Plant Physiology*, **4**, 199 (1953).

⁶ In *Chemistry of Plant Growth Substances* (edit. by Wain, R. L., and Wightman, F.), 3 (Butterworths, London, 1955).

⁷ *Régulateurs Naturels de la Croissance Végétale* (édition du Centre National de la Recherche Scientifique, Paris).

⁸ *Ruhland Handbuch der Pflanzenphysiologie*, **15** (1), 1537 (1965).

⁹ In *Plant Physiology: A Treatise* (edit. by Steward, F. C.), **6A**, 403 (Academic Press, New York City, 1971).

¹⁰ *Science*, **165**, 85 (1969).

¹¹ *Pollen Development and Physiology* (edit. by Heslop-Harrison, J.), 234 (Butterworths, London, 1971).

Announcements

University News

Professor Thomas H. B. Symons, president and vice-chancellor of Trent University, Ontario, has been elected chairman of the **Association of Commonwealth Universities** for 1971-72. **Sir Charles H. Wilson**, principal and vice-chancellor of the University of Glasgow, has been elected vice-chairman and **Sir Douglas Logan**, principal of the University of London, honorary treasurer, for the same period.

Mr R. W. Beard, King's College Hospital Medical School, has been appointed to the chair of obstetrics and gynaecology tenable at St Mary's Hospital Medical School, University of London. **Dr G. Hamilton Fairley**, Imperial Cancer Research Fund Medical Oncology Unit, has been appointed to the Imperial Cancer Research Fund chair of medical oncology tenable at St Bartholomew's Hospital Medical College, and **Mr J. H. Sowray**, King's College Hospital, has been appointed to the chair of oral surgery tenable at King's College Hospital Medical School.

Miscellaneous

Dr R. D. Cannon, University of East Anglia, has been awarded the first **Butterworths scientific fellowship for authors**. He will take up the fellowship in 1972 to write an advanced monograph on electron-transfer reactions. Nominations are requested for the **Lewis Rosenstiel award for distinguished work in basic medical research**, which will be made in the spring of 1972 on the basis of the recommendation of a panel of outstanding scientists in the field. Preference will be given to recent discoveries of particular originality and importance. Further information is available from **Dr H. O. Halvorson**, director of the Basic Medical Sciences Research Center, Brandeis University, Waltham, Massachusetts 02154, USA.

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